

FOURTH REPORT ON A LAMARCKIAN EXPERIMENT

BY WILLIAM McDOUGALL

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PART III. CREW'S ATTEMPT TO EXPLAIN AWAY THE OSTENSIBLE EVIDENCES OF LAMARCKIAN TRANSMISSION AFFORDED BY TABLE II OF MY THIRD REPORT (2)

(1) *Objectivity of experimental results*

ALL the evidence offered in support of the Lamarckian hypothesis by the experiments reported in the present and foregoing three Reports can be attacked in one or other of two ways, or in both. Assuming good faith on my part (without which there would be no case to be criticized) the critic may seek to throw doubt on the objectivity of the figures of my tables, especially those representing the average number of errors per rat for each group. This is the possible line of criticism against which, throughout the earlier years of the work, I was chiefly concerned to fortify my figures. I was perpetually haunted by the doubt—Am I deceiving myself? Am I, in some way I have failed to detect, allowing my bias or interest in favour of a positive result (i.e. one supporting the Lamarckian hypothesis) to influence my handling of the animals in such a way as to produce the appearance of gradual improvement with the successive generations? For, although when I began the work I had no

more than a general inclination to be open-minded towards the hypothesis, an inclination founded on what seemed to me the inadequacy of all alternative theories of evolution and on the opinion that, up to that date, no experiment had been made which had yielded moderately adequate ground for either a negative or a positive conclusion, as the signs of positive results began to accumulate (as in my 16th generation of the water-maze experiment, in my revolving wheel experiment especially, and later in the generations of the tank experiment) I have naturally and, I suppose, inevitably become a partisan, welcoming every new indication of positive results and disappointed when, for example, a later generation of my tank series seemed to show less facility than an earlier one.¹

What has most contributed to give me confidence that I am not the victim of such self-deception is the fact that during the years in which I have been dealing side by side with the later generations of the T.R. series and the 22 generations of the W.H. series, I have been even more keenly interested in the success of the latter than in the success of the former; and that, in spite of this, the former group have continued to show themselves consistently and very markedly superior to the contemporary groups of the latter (the W.H. series), e.g. reaching the low average of 8 errors per rat while their contemporaries of the W.H. 20 group still made 47 errors per rat. And, again, the same is true of the latest groups (those of the early part of the year 1937); the T.Rs. 44 made 9 errors per rat while the W.Hs. made 27 errors per rat.

I have found further grounds for increase of confidence in the objectivity of my figures in many other ways; chiefly in the striking and repeated results of the several cross-breeding experiments, and in the various distinctive modes of behaviour in the tank expressive of increased caution which are closely correlated with excellence of performance in the learning-task.

(2) *Inheritance of 'quickness' and 'slowness'*

But Crew does not take this line of criticism; he does not seek to impugn the objectivity of the figures of my Table II. Rather, accepting them as expressing some very real difference between the later and the

¹ I may say that I have been familiar, in a general way, with the Mendelian genetic work from the days when I was on intimate terms with William Bateson, both of us being resident fellows of St John's College, Cambridge, and the later days when I saw a good deal of the pea-breeding work of Arthur Darbishire, who was my near neighbour on Boar's Hill, Oxford.

earlier generations, he suggests that the improvement of facility expressed by them may have arisen by way of genetic accident. He rightly points out that my neglect to record the pedigrees of all animals represented in the table leaves open the possibility that some one rat (or two or more rats) of one of the early generations may be represented in the later generations very much in excess of its due proportion; e.g. it may be that some one male of the first or other early generation is represented in the genetic constitution of all or almost all the rats of the later generations, while other males are represented not at all or only slightly. Suppose then that 'quickness' and 'slowness'¹ are determined by genetic factors; and, to take the simplest supposition, we assume that 'quickness' is determined by a single gene and 'slowness' by the absence of it, and that 'quickness' is dominant and 'slowness' recessive.² Suppose further, says Crew, that the constitutional or genetic 'quickness' of any rat does not always reveal itself in his behaviour in the tank, i.e. suppose that in some cases the genetically 'quick' rat (by reason of some one or more unfavourable accidental circumstances) makes a 'slow' performance. And he adduces some reasons for thinking that this may occur occasionally, though he has not demonstrated its occurrence in any actual instance. Suppose further that a male of an early generation, whose blood becomes, through the accidents of reproduction in successive generations, widely represented in the later generations of the continuously trained stock (my T.R. stock), is a 'latent quick' (a 'mute inglorious Milton' as it were); then eventually, sooner or later, this 'quick' blood must become manifest in improved performance of all the T.R. stock; the latent genius or 'quickness' cannot go on being latent and disguised for ever; especially as it is, by hypothesis, a dominant genetic quality.

There we have Crew's proposed explanation of the improvement shown in my T.R. stock. What is to be said of it? I think one may say that if, like Dr T. H. Morgan, one knows positively that the Lamarckian hypothesis is nothing more than 'a pernicious superstition', one will welcome Crew's explanation as a highly ingenious way of evading an

¹ It is convenient to use 'quickness' and 'slowness' throughout the discussion as meaning 'quick learning' (i.e. with few or no errors or shocks) and 'slow learning' (i.e. with many errors or shocks) respectively.

² This is the supposition towards which Crew inclines; and, although he admits that 'quickness' may be polygenic, he holds that that possibility does not invalidate the argument. He writes: "These observations suggest that among these rats there are two main classes, the 'quick' and the 'slow', and that in a general way 'quickness' behaves as a dominant in relation to 'slowness'" ((1), p. 94).

embarrassing piece of evidence that seems to point to its validity. But how the improbabilities are piled up in successive suppositions! It must be granted that no one of the suppositions is an impossibility; and therefore the whole chain remains within the bounds of possibility. For, as we know, all things are possible except the self-contradictory. But with every generation that passes without overt manifestation of the postulated latent 'quickness', the improbability of the explanation is multiplied by some considerable figure not easily to be estimated. Now a number of 'quick' performers of the later generations from the 35th to the 44th have made no errors; and these represent we must suppose (in terms of Crew's system of suppositions) the pure effect of the 'quickness' gene unmasked by unfavourable accidents. But no such instance appears before the 35th generation. Throughout 34 generations, then, we are to suppose that the 'quickness' gene, though present in an increasing proportion of the members of successive generations, has had continuously bad luck, has never had a chance to show what it could do, owing to various remarkable conjunctions of adverse accidental circumstances persistently recurring generation after generation and adversely affecting the scores of all rats of each generation which bear this 'quickness' gene, until at last in the 35th generation the spell is broken, the luck changes, and 'genius' appears unmasked.

As to what trait or quality or 'character' of the rat this hypothetical quickness gene determines or produces or conditions, Crew is not very explicit. But he clearly inclines to suppose it to be what he calls 'photophobia'. And much of his experimental work is directed to throw light on this supposed trait.

Before discussing further this 'photo-phobia', I must point out some peculiarities of Crew's procedure which artificially favour or give appearance of plausibility to his supposition of a 'quickness' factor and a 'slowness' factor or gene as the all-important conditions of 'quick' and 'slow' performance.

Clearly, if the supposition is well founded, we should expect to find some indications of the rats falling into two groups, the 'quick' and the 'slow', even though it be admitted that we cannot expect the rats of every group to fall into two clearly distinct divisions, the 'quick' and the 'slow'. Unfortunately we are not dealing with simple qualities, observable at a single glance, such as the geneticist is accustomed to deal with; but rather with a quality that can be observed and measured only by a long and tedious process repeated day after day and subject in its manifestation to many disturbing influences. So far Crew is right;

one must freely concede that these influences may well obscure the distinction between 'quick' and 'slow' and cause the appearance of wide overlapping of the two supposititious groups. Now in my 209 control rats there appears no evidence of the presence of any one 'quick' performer. The best performer of all made 14 errors, and very few made fewer than 50 errors. This seems to show that the genetically 'quick' rat must be a rare bird in the control stocks; and the chance of one or more of them having been included in the small parent group of my T.R. stock must therefore have been very small. But Crew will reply that 'quick' rats were not infrequent among both his controls and his experimental series, namely, in the former, 10 rats and, in the latter, 29 rats which made no error and which therefore were pure 'quicks'. Now I have already adduced very strong evidence to show that Crew's procedure makes the task of the rats in his tank much easier than under my conditions. And there is a further feature of Crew's procedure (not hitherto mentioned here) which favours greatly the appearance of zero-performers and also favours the appearance of great 'slowness' in other rats. Namely, Crew, very properly wishing to throw light on the phenomenon of 'handedness' (or tendency to turn predominantly to right or to left) and also of 'photophobia', modified my standard procedure as follows: in place of my zero-day trials (6 immersions of each rat with alternating light but no shock), he put phases 1 and 2 of his training, phase 1 consisting of 50 immersions with only dim equally diffused light and no shock; phase 2 consisting of 50 immersions with alternating bright light but no shock. Phase 3, involving alternating light and shock, was thus preceded by 100 immersions in place of my 6 immersions of zero-day.

Now, as Crew shows by examples, some rats during phase 1 go habitually either to left or right; that is to say they acquire a side-habit, which in many cases is further confirmed by the 50 immersions of phase 2. But a side-habit, once formed, is, as I have repeatedly found and as Crew agrees, very unfavourable to learning. Here, then, we have the explanation of the fact that among Crew's rats (although the task set them by his procedure and tank is very much less difficult than that set by my procedure and tank) are many which make a number of errors nearly as high as that of my slowest rats, i.e. in the neighbourhood of 300 errors (but not quite so high, for Crew's highest number of errors for any one rat, out of some 2500 rats, is 307; while among my 209 controls are a number of rats which made well over 300 errors, a few going even beyond 350).

On the other hand, some of Crew's rats during phase 2 went pre-

dominantly or always to the dimly lit exit *D*, and thus, through some 50 repetitions, confirmed into a habit what was at the outset a *very slight preference* for *D*. And this habit is of the opposite effect from the side-habit: when its possessor goes on to phase 3, he has but to continue the habitual routine of going to *D* which he has acquired in phase 2, and he takes his place in the list of those that have qualified as having passed the test without error, have got through the ordeal without once receiving the shock.

We thus see why it is that, as Crew reports, his figures for errors made seem to "suggest that among these rats there are two main classes, the 'quick' and the 'slow'", whereas my figures convey no such suggestion, but suggest rather completely continuous gradation of facility from the quickest to the slowest.¹ For Crew, by means of his procedures of phase 1 and phase 2, has manufactured his two classes, the 'quick' and the 'slow'; or (if that is putting it a little too strongly) his procedure has accentuated the natural quickness of some and also the natural slowness of others, and has thus thrown a considerable proportion of the whole number to one end of the scale and another considerable proportion towards the other or slow end of the scale.²

(3) 'Photo-phobia'

Crew is open-minded as to what is the genetic quality (or complex of qualities) which he would attribute to the 'quick' rats as the ground (or principal ground) of 'quickness'. But in the main, it would seem, he inclines to regard it as 'photo-phobia'. Accordingly he inclines to postulate 'photo-phobia' as an all-or-none Mendelian dominant quality. But he admits that the evidence of this is none too clear. Whenever a rat, in phase 2 of his procedure, shows large predominance of *D* choices he calls that rat 'photo-phobic', and attributes to it the postulated quality or Mendelian trait 'photo-phobia'. But, as he somewhat plaintively remarks, after describing his endeavours to make manifest this trait: "It becomes a matter of some difficulty to demonstrate that any given rat is definitely photo-phobic" (1, p. 83). Perhaps the difficulty

¹ I give here for what it may be worth as evidence bearing on this point the error-scores of a batch of 35 control rats obtained from the Wistar Institute in 1932, which were recorded individually with a view to initiating a new series: 14, 16, 23, 26, 34, 37, 38, 38, 52, 52, 52, 53, 57, 58, 59, 59, 77, 91, 96, 101, 107, 108, 114, 118, 120, 125, 133, 137, 149, 158, 163, 206, 213, 223, 344. This batch was very unusual in containing the two rats with the very low scores 14 and 16; nevertheless the distribution runs pretty smoothly, forming a curve with a single hump near its middle rather than one humped near its two ends.

² Cp. pp. 383, 384 for a second way in which Crew has produced this artificial grouping.

is that there is no trait which can properly be named photo-phobia. However he adds: "Nevertheless the difference between the behaviour pattern during phase 2 of these rats [i.e. those which give large predominance of *D* choices in phase 2] and of the rest is such as to make it quite certain that they are avoiding the light as light to begin with and thereafter continue to alternate as a habit [still taking always the *D* route]." And accordingly he remarks: "The average score of a given batch of rats must necessarily be influenced greatly by the number of such photo-phobic rats within it, and so in any experiment of this kind it is necessary to include a test for photo-phobia. It is incumbent upon McDougall to show that the explanation of his observation that his tank-trained stock tend to avoid the bright gangway whereas his controls tend to prefer it, is other than that through unconscious selection there are now many more photo-phobic animals among his trained stock than among his controls" (p. 84). Now it was exactly with this object in view that I initiated and have carried through the 21 generations of adverse selection combined with training in the tank (cp. Table X, in this *Journal*, xxviii, 331). But before turning to examine Crew's scanty comments upon this my crucial experiment, I wish to examine a little more closely this alleged photo-phobia.

I have already pointed out in Part II of this paper that the conditions of illumination in Crew's tank seem to be such as to evoke some 'unconditioned' aversive reaction from the bright route in a larger proportion of rats than do the corresponding conditions of my tank: and, one may add, presumably a stronger aversive reaction in many of them than is evoked in any of mine. For it is, I think, clear that this aversive reaction (whether or not it is properly to be called a fear-reaction) is of different degrees of strength or intensity, as the fear-impulse obviously and notoriously is under varying circumstances that evoke it. That is to say, the photo-phobic reaction is by no means a simple all-or-none reaction. Under the conditions of my tank it is very obvious that in nearly all, perhaps all, cases in which aversion from *B* (and preference for *D*) is manifested on the zero-day, we are dealing, not with any strong over-riding factor, but rather with a factor which is of the weakest possible kind and just suffices in a certain proportion of instances to turn the otherwise even balance between tendencies to go to *B* and to *D*. That this balance is very even in the control stocks is shown by the fact that the total choices of *B* and *D* on zero-days are almost exactly balanced; and the same is true of all but the later generations of the T.R. series. Now it might be that the even balance of the

total figures for *B* and *D* choices on zero-day in these stocks was maintained in spite of photo-phobia (in some animals) sufficiently strong to make them go always to *D*; the opposite trait, 'a-photo-phobia' or 'scoto-phobia' being present in an equal proportion of the animals. Now I could make absolutely certain statements on this head only if I had kept the individual zero-day records of a large number of the animals. Unfortunately I have not done so; but I have observed a large number of animals very carefully during zero-day performance, and I feel sure that if there had been any considerable number of animals showing marked preference for *D* or *B*, I should not have failed to detect them, especially in the earlier generations of T.R. and the controls of that period; for at that time just such aversion from *B* on zero-day was the sign or symptom which I was specially keen to observe and record if possible as evidence of some transmission of the effects of training from generation to generation. I was, throughout the training of the first 13 generations, relying upon manifestation and observation of such preference as the evidence of any such transmission as might occur.

Further, when in the last 12 generations my interest in such zero-day preferences was revived by the appearance of a predominance of *D* choices on zero-day (such as I had in the earlier generations carefully looked for and failed to find in any marked degree) I was interested to note how it was manifested, of what nature it might seem to be. I can assert that in very many instances of rats manifesting some predominance of *D* choices the predominance is not complete and exclusive; only in the rarest cases in my tank does any one rat avoid *B* completely on zero-day or on following days. In nearly all cases one or two *B* choices are made. Further, the behaviour in the water of the rats on zero-day (and especially of rats of these later generations which show well-marked predominance of *D* choices) is such as to suggest, not any strong and decisive aversion from *B*, but rather a very faint aversion from *B*, just sufficient to turn the balance in favour of *D* in a majority of the zero-day trials. Here I repeat that I have habitually repeated the zero-day trials on a second and a third zero-day, when some marked preference was shown on the first zero-day. The most common behaviour on zero-day is that *R* swims from *O* straight to some point on the curved end-wall of the tank (which on one side is illuminated by the light from the bright gangway). Arrived at the end-wall, *R* swims either rightward or leftward along the curved wall (maintaining contact with it) for a few inches; then reverses his direction and swims a few inches in the opposite direction, and repeats, oscillating thus in short arcs along the end-wall. Very

frequently the reversal of direction takes place when *R*'s head enters the illuminated zone or area of the end-wall. The oscillations generally become wider in range until eventually *R* reaches and touches one or other gangway and forthwith emerges. That is to say, the behaviour suggests a balanced pendulum swaying from side to side until some slight factor, like a puff of air, upsets the balance and the pendulum falls to the corresponding side.

Further, I am strongly of the opinion that, in so far as any rat manifests aversion from *B* in my tank, the aversion is not the manifestation of an aversion to light in general, such as might perhaps be properly described as photo-phobia no matter how faint the aversion; but rather is specific to the specific situation, namely the panel of light at the end of the passage. I cannot at present prove this, but there is much in the behaviour of the rats to suggest it; and we know from the work of Pavlov and his school and of J. S. Lashley that animals (dogs and rats more especially) can be conditioned to respond specifically to patches of light of particular shapes and sizes, or to other spatial arrangements. And in instinctive behaviour such specific reactions to very specific visual and auditory (or other sensory) patterns are the rule, although authors have grossly neglected this aspect of instinctive behaviour in favour of a quite undue emphasis upon constancy of motor reaction.

All of these observations on the *D* choices of zero-day point to the aversion from *B* as a slight factor varying indefinitely in strength from one animal to another, just as it varies in strength in any one animal in training, growing gradually stronger with repetitions of the shock-experiences, until it becomes strong enough to determine complete avoidance of *B* and a consequent preference for *D*.

No unbiased onlooker will, I think, admit that Crew has demonstrated the occurrence of photo-phobia in any other sense than that which is implied in my foregoing account of the avoidance of *B* shown by my rats. That a general photo-phobia, even of the faintest kind, occurs as a genetic trait in the control stock has yet to be demonstrated; although in the albino rat with its unpigmented eyes it might well be expected to be manifested in some degree by all otherwise normal animals. Still less has it been demonstrated that photo-phobia occurs as a Mendelian unit-trait, manifesting itself on the all-or-nothing principle, segregating in the cross-bred progeny and showing dominance or recessiveness. Crew's suggestion of such photo-phobia is supported only by the distribution of his cases in a curve that tends to be humped at both ends; and,

as shown, above, this humping is just such as his procedure, with its three phases, must tend to produce.¹

(4) *Other explanations of 'quickness' and 'slowness'*

But Crew has other possibilities of explanation up his sleeve to be produced if photo-phobia fails to appear plausible as the means of explaining away all my evidences of transmission.

On the very slight basis that he has found runts which learn quickly, Crew suggests that "the more vigorous the rat the higher the score may be expected to be; and so in comparing the performances of the later with the earlier generations it becomes necessary to show that in respect of general vigour there has been no steady deterioration" (1, p. 92).

Fortunately I have had this question of vigour much on my mind from the outset. I abandoned the water-maze experiment after 16 generations of very laborious work with it, just because it had become clear that lack of vigour was very unfavourable to good performance and that fluctuations of vigour from generation to generation are apt to occur in spite of all efforts to maintain the stock at its fullest health and vigour. Such changes of vigour have occurred in my T.R. series and, in the light of much experience, I agree with Crew that very great vigour and liveliness is a little unfavourable to quick learning in the tank, and a somewhat diminished or less-than-average vigour is probably slightly favourable; although I have had some runts which have learned slowly, as well as some which have learned quickly. I attribute the adverse influence of great vigour and liveliness, not, as Crew does, to the more vigorous rats getting less of the shock; for, under my method of controlling the strength of the induced shocking current, the current is made slightly stronger for a particularly vigorous litter, so as to secure a due proportion of tetanizations. I attribute it rather to the fact that the lively and very vigorous rats are apt to rush at *B*, whereas rats below par in vigour commonly approach *B* more slowly. In the former method

¹ I have to point out on a later page (p. 383) a second feature of Crew's procedure which must tend strongly to produce just such distribution. It is consistent with this view of the so-called 'photo-phobia' manifested by Crew's controls in phase 2, that, according to Crew's own showing, a rat may manifest strong preference for *D* during phase 2 and yet prove in phase 3 very slow to learn. Compare Crew's fig. 7 (1, p. 89); this records the performance of a particularly interesting rat. This animal, in spite of being 'photo-phobic', and having no side-habit, was nevertheless slow to learn. Alongside and contrasting with this case, consider Crew's record in fig. 10. This rat formed a strong side-habit in phase 1, showed no trace of 'photo-phobia' in phase 2, yet learnt with only 3 errors in phase 3. Such contrasting cases surely show that the factors on which Crew lays so much stress are of minor importance only.

of approach, the mere physical momentum carries the rat rapidly over the most critical stage of approach, that in which discrimination is achieved; while the slower approach of the less vigorous rats gives more opportunity for the full development of the conflict of impulses which is a necessary condition of discrimination and, I think one may say, of all judgment, whether in rats or men. However, be that as it may, the superior 'quickness' of my later T.R. generations cannot be attributed to a general decline of vigour, for the decisive reason that the superiority has been manifested not only throughout waves of decline of vigour, but also throughout waves of great vigour. Some three years ago I was seriously concerned about decline of vigour of the T.R. stock. There was deficient fertility, small size of litters, small size and weight of most or all the rats, and such degenerative symptoms as neglect of litters, killing of the young and sometimes eating of them on the part of the mothers. Happily this phase has passed, and my last generation especially, T.R. 44, was quite remarkable for the vigour and full health of practically all members. The litters were large, running in several cases to twelve or thirteen members, the general appearance, especially of the coats, was excellent, the size of the individuals at all stages of growth compared very favourably with prior generations and with control-litters, a point established by weighing them and control-litters at 60 days after birth. Finally, they behaved in the tank in a lively and vigorous manner. Yet, in spite of all these signs of unusual vigour, they made the very low average of rather less than nine errors per rat, very nearly equal to the record of the best generation, T.R. 38, which made a little more than eight errors per rat. I confidently submit, then, that, though changing degrees of vigour may be the ground (or one of the grounds) of fluctuations of the average-error-figure from generation to generation, it is not in any important degree the ground of the sustained superiority of the later to the earlier generations of the T.R. series.

Another suggestion is that the strength of the current in my procedure may have increased in the later stages sufficiently to account for the improvement. I submit that my method of controlling the strength of the current in terms of its effects on the rats (while it may or may not be superior to a physical standardization, as I hold it to be) gives sufficient guarantee against any such large change as could account for the improvement; especially in view of Rhine's experimental demonstration ((3), p. 211) that, though a current markedly stronger than that which just suffices to tetanize in half the cases does favour quick learning, the difference of effect is not very large.

In summing up his conclusions Crew remarks:

I submit that there is no need to postulate, in explanation of the fact that the average scores of the earlier and later generations of McDougall's rats differ, that some new quality has been acquired and is being transmitted, for the average score of a generation is determined by the proportion of quick and slow rats within it.

I submit in reply that this assertion flies directly in the face of evidence afforded by my Table VII (this *Journal*, xxviii, 323), and supported by all my other tables, to the effect that we are not dealing with two sharply different kinds of rats, the 'quick' and the 'slow' (those with the hypothetical dominant gene *X* and those without it), but rather with rats which at all stages of the experiment differ from one another by an infinity of small steps in respect of facility in this task. Consider in this connexion the figures giving the performance of the worst rat in each generation of Tables VII and VIII (*loc. cit.* p. 326). Among the 12 control groups the best slowest rat of his group makes 124 errors. In the upper quarter of the Table VII comprising the first eight of the generations there recorded the slowest rat figures range from 74 to 147; in the second quarter they range from 62 to 105, in the third from 56 to 124, in the fourth from 25 to 78. If, as Crew asserts,¹ the rats were all of one or other of two types, the 'quick' and the 'slow', we should expect to find either a fairly constant figure for the worst rat throughout the series, or (if in the later generations all rats are 'quicks') a sudden fall of the figure for the slowest rat from something well over 100 (as in all the control groups) to some such figure as 25 or 30. Table X (*loc. cit.* p. 331) displays a similar gradual decline of the figure for the slowest rat, without the slightest hint of the existence of the two genetic or gene-determined and sharply different types, the 'quick' and the 'slow'. In this connexion it is to be noted that in all Crew's 18 control groups the figures for the slowest rat of each group range from 81 to 298, and the corresponding figures of the experimental group range from 47 to 307, suggesting strongly a wide continuously graded range of facility, rather than a specific quality of genetic slowness.

(5) *The effects of favourable and adverse selection*

Let us now see what Crew has to say of my adversely selected series which was instituted for the purpose of ruling out the possibility of

¹ "If the average score per rat of one generation differs from that of a preceding generation, then this difference merely means that in the two generations there are different proportions of genetically quicks and slows" (1, p. 97).

improvement of facility through unwitting favourable selection; or chance predominance in the later generations of the blood of ancestors of superior facility. The experiment is twofold: in the W.H. or 'worse half' series the better performers making up half the trained group of any generation are discarded, while the other half (made up of the worse performers) is kept for breeding the next generation. And this is supplemented by the parallel B.H. series, in which of the trained group of each generation the better half alone is kept for breeding, and the worse half discarded. Tables X and XI (*loc. cit.* pp. 331, 332) tell the story of these two series.

Crew makes no mention of this twofold experiment, to which I attach the greatest importance, until he comes to his final paragraph. There he writes:

It has to be acknowledged that this suggestion of a single main factor pair (or of some system of multifactor inheritance giving similar results), though satisfactory up to a point, does not accommodate the fact that a considerable number of quicks appear among the offspring of slows $\times$ slow-matings. It is also in conflict with the results of McDougall's subsidiary experiment in which training was combined with deliberate adverse selection (breeding consistently from the slowest in each successive generation) and with favourable selection (breeding consistently from the quickest). In the case of adverse selection McDougall got remarkable improvement after 14 generations, whereas with favourable selection no such results were obtained. He regards this ineffectiveness of adverse selection as the strongest possible corroboration of his main conclusions.

Here I interrupt Crew's comments to make two remarks on them. First, it is not the ineffectiveness of adverse selection which I regard as corroboration of the indication of Lamarckian transmission afforded by my T.R. series; it is rather the *effectiveness of training continued through many generations in spite of continued strongly adverse selection*. Secondly, Crew is not warranted in taking seriously, as he proceeds to do, the fact that my B.H. series, unfortunately cut short by infertility, showed no clear-cut improvement. For not only was the series very short, but also the numbers concerned were (owing to infertility) absurdly small. And my new favourably selected series (B.H. 2 of Table XI) does show very distinct improvement in the course of 10 generations of training, almost exactly the same degree of improvement as is shown by the corresponding 10 generations of the W.H. series (cp. Tables X and XI). It shows also the appearance of the predominance of *D* choices on zero-days: if we add together the figures for *D* and *B* choices of the last 7 generations of Table X and of the last 8 generations of Table XI (getting thus nearly

equal numbers of rats from the two series) we have the figures 663*D* to 537*B*, and 682*D* to 554*B*.¹

Further, I insist not only on the ineffectiveness of adverse selection to counteract the improving effect of training in successive generations, but also upon the even more striking ineffectiveness of favourable selection through 10 generations to improve the facility of the stock.

Crew goes on (11, p. 95) to offer an explanation of the fact that selection, both favourable and unfavourable, seems, according to the evidence of my Tables X and XI, to be of very little effect (I do not say of no effect, since some adverse influence of adverse selection seems unmistakably revealed by Table X when compared with Table VII). He writes:

But surely there is a very simple explanation—It is far more difficult, in my opinion, for a constitutionally 'slow' rat to pass through the test with a low score than it is for a constitutionally 'quick' rat to put up a high score. Quite a number of happenings, casual or accidental, can convert a potentially low score into a high one, and so the 'slows' come to include a number of 'quicks'. For this reason I would expect the quicks to be relatively uniform and the slows heterogeneous. Thus if 'quickness' and 'slowness' were genetic characters, in whole or part, and polygenic in nature, and if selection of the worst implied the selection of individuals of different genotypes whilst selection of the best meant selection of a more uniform genotype, I should not expect favourable selection to be followed by a drop in the arithmetical mean.

Up to this point I agree, except that I do not accept the Mendelian implication of Crew's terms 'quicks' and 'slows'. I can at least follow the reasoning up to this point, and there is nothing in it adverse to my interpretation. But the whole of Crew's attempt to explain away the

¹ In this connexion I take exception to Crew's remark: "I very much doubt, however, that McDougall is warranted in basing any conclusion whatsoever on the behaviour of the rats during six runs with alternating light and no shock" (p. 85). I have already said that, wherever a marked predominance of *D* choices appeared on zero-day, I have made a practice of repeating twice over the zero-day trials. But disregarding that, I maintain that, although six runs in the case of any one rat is too small a number, standing alone, to be of any significance, yet when the predominance of *D* choices appears in the records of many rats it becomes significant and would be significant if each rat had made but a single run. The significance I insist upon, against Crew's judgment, is that of the facts (1) that in all my control groups taken together there is a predominance of the *B* choices (namely 612*D* to 642*B*); (2) that both Tables VII and X show a similar predominance of *B* choices throughout their earlier generations (first 8 generations give 374*D* to 406*B* and 292*D* to 305*B*); (3) that it is only in the last few generations of the two series that predominance of the *D* choices appears (last 9 of Table VII summated give 740*D* to 472*B* and last 7 of Table X give 663*D* to 537*B*). Further, Table XI gives similar predominance of *D* choices. It is this reversal of the proportions of *D* and *B* choices in the records of large numbers of rats which I insist on as significant.

Lamarckian indications of my Tables X and XI is contained in the remainder of this obscure and complicated sentence. It runs as follows: "*but I should expect that this might easily happen if adverse selection were practised, since in selecting 'slows' one might be choosing potentially 'quicks'.*".

I confess myself completely baffled by this sentence. Unless I permit myself to indulge in psychological interpretation of this sentence, I can only say that either my brains are addled or Crew's must be. I submit that, all his premises being granted, all he is entitled to argue is that continued favourable selection might sooner or later produce a genetically homogeneous stock which, being made up of 'quicks' alone, *under further selection of the same favourable kind*, would show no further improvement; and that, on the other hand, continued adverse selection would not so readily or so soon produce a homogeneous stock consisting of 'slows' only, because in each generation some disguised or masked 'quicks' *might* remain in this stock for many generations. Suppose that by an effort we grant the possibility and even the probability that some masked or latent 'quicks' continue to occur in all my 24 generations of the W.H. series. The result must be that in spite of my adverse selection, in each generation some of the hitherto latent 'quicks' will break out with overt quickness, and, in this way, prevent the achievement of a uniform level of 'slowness' such as a pure line of 'slows' would manifest. That is to say, it would follow that favourable selection should much more readily produce a maximum improvement after which it could produce no further improvement; whereas adverse selection might fail to achieve the maximum possible falling off of performance, leaving always the possibility of some further effect of selection in the same direction, a further falling off. But by some strange mental operation Crew has twisted out of the premises the conclusion that adverse selection may be expected, not only to be less effective than favourable selection in producing its maximum effect, but actually to produce the opposite of its inevitable effect, to produce improvement rather than falling off of facility. This at least is the only intelligible interpretation I can put upon Crew's obscure phraseology. When he says "I should expect that this might easily happen if adverse selection were practised", the word 'this' can mean nothing other than 'a drop in the arithmetical mean'; and the phrase "I should expect that this might easily happen" must mean that the occurrence of this has in his opinion considerable probability. Translated, then, into less ambiguous language, Crew's statement amounts to this: it is a fair probability that continued adverse

selection may produce continued improvement of facility. Now if my adverse selection consisted in picking out for breeding purposes (from each generation after training) a few rats from among many 'slow' ones, it would in each generation be a bare possibility (but wildly improbable) that I should happen thus to pick out 'disguised quicks' rather than the 'slows'; and, further, that, if this should by some vastly improbable chance be repeated generation after generation, it might result that the attempted adverse selection should prove to be in reality a favourable selection and improvement of facility might result instead of continued falling off. But the improbability of this continuing to happen by sheer chance generation after generation must mount up to astronomical figures after some few generations.

Further, what is the probability that of the five ancestors, or of the two males, from which all the W.H. and B.H. rats are descended, one was a 'masked quick'. It is to be remembered that all five of these animals (the five slowest of a group of 37 rats) made in the near neighbourhood of 200 errors apiece. There is clearly a very strong probability that all of them, and especially both the males, were true 'slows' (if we accept for the sake of the argument Crew's improbable assumption of the 'quicks' and 'slows'). Thus his argument starts out with two ill-founded and improbable assumptions and continues to pile one improbability on another with every one of my 24 generations of adverse selection, and concludes that what is at the most the barest theoretical possibility, but in reality a vastly improbable result, is what Crew would expect *might easily happen*; it is in fact what he suggests has happened in the case of my W.H. series.

I submit then that, while Crew's explanation of the improvement shown by my T.R. series is, though very improbable, not to be rejected as beyond the realm of possibilities to be seriously considered, his application of the same explanation to my W.H. series does go beyond the limits of that realm. Mere duplication or repetition of the T.R. series would go far to make his explanation in terms of crypto-quicks too improbable to be seriously considered; just as a run of ten heads on tossing a coin may, without too great straining of probabilities, be put down to chance, while a second similar run following immediately upon it would compel any reasonable man to look for some other explanation. But when, to the improbability of the repetition of so improbable a result, is added the improbability that, throughout my 24 generations of adverse selection, crypto-quicks continue to escape elimination by remaining cryptic and simulating 'slows', and actually do so in such numbers as to become

increasingly numerous in proportion to the true slows in the successive generations,¹ we reach an extreme degree of improbability approximating to an impossibility.

In regard to the bearing of my B.H. series, I admit that Crew's argument as to the possibility of some crypto-quicks remaining through some generations within the W.H. stock does weaken my contention that the B.H. series shows how little selection can do within so inbred a stock, one so relatively homogeneous in respect to the quality selected favourably or adversely; for it remains possible, in view of this possibility, that favourable selection might work more successfully on the W.H. than upon the B.H. stock. But this would be true only if and when the B.H. and the W.H. stocks had become widely differentiated by generations of favourable and adverse selection respectively. The striking fact about the B.H. stock is that from the first, i.e. prior to any such differentiating selection which might be supposed to have eliminated from it all 'slows', it obstinately continues closely parallel (in respect of performances of its generations) to the W.H. series, showing no clearly marked superiority to it.

I submit that this close parallelism between the B.H. and the W.H. series, in spite of the strongly favourable selection practised on the former and the strongly adverse selection practised on the latter, amounts to very strong evidence: first, that the parent stock from which both derive was not composed of a mixture of genetic 'quicks' and genetic 'slows'; for if that had been the case some marked differentiation must have resulted from 10 generations of the favourable and unfavourable selection practised on the two stocks (and this is true even though the occurrence of many crypto-quicks be granted as probable); secondly, that very marked deliberate selection can do but little to modify such closely inbred and relatively homogeneous stocks, and therefore it remains highly improbable that such a large change (improvement of facility in the tank) as is shown by the T.R. series within 44 generations can be due to some accidentally favourable selection recurring again and again throughout the series.

If Crew could show that high fertility is correlated with 'quickness' and low fertility with 'slowness' his suggested explanation would have a little more respectability. Accordingly he does make some slight move in that direction; for he is justly determined to leave unturned no stone

¹ It is to be remembered that Crew's proffered explanation of all improvement of facility shown by any group is the increase of 'quicks' in proportion to the number of 'slows' contained in the group.

that might contribute to make possible avoidance of a Lamarckian interpretation. He remarks that his "figures seem to suggest that quickness and fertility are connected in some way"; but honourably adds: "I think they are misleading." He also adds: "But it is the case that I have never had any offspring out of a rat with a score of 200." To the last statement I can oppose the instance of the five rats (of Table I in my Second Report⁽³⁾) which became the ancestors of my W.H. series. They ranged from 204 to 229 errors and all were markedly fertile. The three females in particular, after producing large litters to the two males of this small group, afterwards were mated with T.R. males; and at once all three again produced large litters. I may say that I have kept my eye open to evidence on this question throughout, and have observed no indication of positive correlation between 'quickness' and 'fertility'; on the contrary I have collected some evidence of the opposite correlation, namely of quickness to learn with infertility. In the course of my water-maze experiment, which was continued through 16 generations, I attempted several times, as a side show, to isolate a stock of superior quickness by breeding very quick males with similar females, hoping thus to produce a 'genius stock'. And each time the attempt failed through lack of fertility of these superior rats or of their progeny. Indication of the same kind is afforded by the dying out through infertility of my first favourably selected series of the B.H. experiment in spite of all my efforts to perpetuate it.

(6) *Bias shown by Crew's criticisms*

In spite of the admirable willingness of Crew to repeat my experiment (with unfortunate modifications) and in spite of the invariably courteous and moderate tone of his article, this is definitely an attack on me; and I find that, like the wicked animal of the Frenchman, I have the disposition to defend myself when attacked. I therefore carry the war into the enemy territory by charging Crew and the geneticists in general with a strong and ill-founded bias in this question of Lamarckian transmission. Crew's adverse bias is I think obvious to any impartial reader. It appears most clearly in the obscure passage on p. 95 of his article, which I have already cited and examined at some length (culminating in the phrase: "I should expect that this [improvement of facility] might easily happen if adverse selection were practised"), with which he most inadequately and cavalierly and confusedly dismisses the crucial evidence of my adversely selected series.

² But it is shown also in his almost total ignoring of the records of my

controls and of the evidence afforded by them that the degree of facility of the parents of my T.R. stock cannot reasonably be estimated to be lower than would be expressed by an average of 100 errors per rat and in all probability not lower than 130 errors. It is shown in his total ignoring of the overwhelming evidence that the task presented by his tank and procedure is definitely less difficult than that presented by mine. It is shown especially in his attempt to dismiss the almost startling improvement shown by the seven generations of the Scottish controls of his stock trained by Rhine and myself. He remarks that he cannot admit evidence of improvement and adduces the following reasons. First, because in the case of such small batches "the average is disturbed too profoundly by the presence therein of one or more animals with very high scores". He assumes this to be the ground of the high score of 188 errors made by the first generation of 11 rats. But as a matter of fact there were, among the 11 rats, four which made high scores (namely 240, 264, 270, 282) and two which made moderately high scores (namely 150 and 168); and the anomalous feature of this batch was the one rat which learnt with only 16 errors, a performance equalled by only one other rat among all my 209 controls. Thus the high score of 188 errors is not due to the presence of one or two extremely slow rats; and it would have been higher but for this very unusually quick performance of one member of the batch.

Secondly, Crew argues that he may legitimately ignore the first two generations of my Scottish controls:

If the first two generations of my rats in Durham [N.C.] are disregarded, and it is surely fair to do this for the conditions there must necessarily be different from those in Edinburgh and rats undoubtedly require a little time to become acclimatized, then there is no difference whatsoever between the actual figures McDougall got and mine.

Now I have admitted that *possibly* the performance of the group S.C. 1 was unduly poor owing to the long journey which preceded the training (although, as Crew and I agree, some lack of vigour is favourable rather than adverse to quick learning). But on what ground does Crew dismiss also the second generation? On the ground that the process of acclimatization continues through two or more generations. It would seem that Crew, in his ardour to explain away evidences of Lamarckian transmission, is prepared in the service of that noble cause to postulate the reality of Lamarckian acclimatization.

I add one more crowning manifestation of Crew's bias. We have seen that his whole argument against my evidence is based on the assumption that every rat is either a genetic 'quick' or a genetic 'slow', and

that differences between one batch and another in respect of scores (or average number of errors) are due to differences in the proportions of 'quicks' and 'slows' among the members of the two batches. And we saw that he supports this by adducing as evidence in its support figures which "suggest that among these rats there are two main classes, the 'quick' and the 'slow'". We saw also that he ignores the fact that his procedure, with its two preliminary phases, must tend to produce just such an appearance of "two main classes". I have now to cite a still more damning evidence of bias. Namely, Crew himself tells us that, throughout his handling of the 18 generations of his experimental stock, he was mating in the main 'quicks' with 'quicks' and 'slows' with 'slows', and that thereby he developed a quick line (*A*) and a slow line (*B*). Referring to his Table VI, he writes:

It will be noticed that there is a preponderance of quick $\times$ quick matings in line *A* and of slow $\times$ slow matings in line *B*. Quick $\times$ slow matings are rare; they were made only when it was not possible to mate quick with quick or slow with slow.

Thus Crew's procedure has produced in two ways the appearance of two genetic types, the 'quick' and the 'slow'; of one of these ways he seems to have remained unaware; the other was deliberately practised with full knowledge of its probable effects. And Crew having thus produced by two peculiarities of his procedures this appearance of two types as artefacts, cites the evidence of the existence of these artefacts as evidence that all rats fall naturally into two such classes or types and makes this the foundation of his explaining away of all my evidences.

One last evidence of bias. I have already examined Crew's remark: "I very much doubt that McDougall is warranted in basing any conclusion whatsoever on the behaviour of the rats during 6 runs with alternating light and no shock." I have pointed to its utterly fallacious quality. I submit now that only a strong bias could lead so intelligent a man as Crew to make this remark.

PART IV. OTHER VIEWS ON THE LAMARCKIAN THEORY

(1) *Criticisms by T. H. Morgan*

Crew is not alone among geneticists in manifesting an ill-founded bias against the Lamarckian hypothesis. Dr T. H. Morgan, commonly recognized as geneticist-in-chief, describes it as "a pernicious superstition". It is perhaps worth while to mention the grounds offered by him for this characterization and for his rejection of my evidence. In

his *Scientific Basis of Evolution* (New York, 1923), he writes: "The new work in genetics has struck a fatal blow at the old doctrine of inheritance of acquired characters—this already frail and mysterious doctrine." He cites a few instances in which some recessive trait appears from time to time quite unlike the corresponding trait of the ancestry, e.g. vestigial wings in a line of long-winged ancestors, or the colours of the Andalusian breed of fowls, and adds: "These will suffice to show that the character of the parents does not affect the properties of the germ cells." And at once the question arises—*How suffice* except for a highly prejudiced mind? He goes on to write of the story of Lamarckism as "a veritable nightmare of false logic, of insufficient evidence, of mistakes of many kinds, and of sensationalism rampant". Are not similar strictures applicable also to the earlier stages of all branches of science, notably to physics, chemistry, astronomy, and geology? Of my experiments, he writes:

The results have every appearance of some kind of selection [but does not indicate what these appearances are]. What kind of change took place is not apparent. There is nothing to indicate that the effects were due to a specific kind of education connected with escape from water through a choice of exits. Until the kind of change that took place in the rats is more evident, it is not possible to discuss to advantage the outcome of this work.

I submit that it is now made clear that a principal, and perhaps the essential, change in the later generations of my continuously trained generations is an aversion from (or an increased readiness to manifest avoidance of) the brightly lit gangway, just that kind of change which all the individuals most obviously acquire in the course of their training. It is true that though we (Morgan himself included) talk about the acquirement of 'conditioned reflexes' as though that were some well-understood process, it remains true that no one knows what changes in the organism underlie the 'conditioned reactions'; yet that is not commonly held to be good reason for ignoring the evidence that such changes are produced by the appropriate course of training, nor for asserting that there is "nothing to indicate that the effects were due to a specific kind of education". In another place (4), Morgan writes:

If newly acquired habits of an individual should cause corresponding structural and psychic changes in its offspring, heredity would become a branch of animal psychology. [Precisely! This prospect no doubt would fill Dr Morgan with dismay. And yet biology remains in a blind alley so long as it continues to ignore the mental activities and dispositions of the individual and of the species.] The slogan [why slogan?] that instincts are inherited has, it is true, at times held the attention of

philosophers and even scientists of the old school [including Charles Darwin]. Lamarck, the acknowledged founder of the doctrine of the inheritance of acquired characters brought forward no sufficient evidence in support of his views, since the cases he cites by way of illustration—the giraffe's long neck as a result of stretching, the membranes of wading birds from spreading the toes in mud, etc., are largely figments of the imagination. In fact the idea itself did not originate with Lamarck, but is a very ancient one. A belief in such transmission is found in the folk lore of many peoples, and Lamarck's evidence is indeed as naïve as that of the stories and myths. It is, I think, largely because Lamarck relied upon argument, rather than objective proof, that his theory failed to be widely accepted by physiologists trained in the more exact methods of science, and found its adherents largely among artists, amateurs, palaeontologists, psychologists and zoologists not addicted to the rigors of experimental science.

And in the book cited above he writes in general terms that "biologists have come to reject the theory".

I submit that, while Morgan's remarks on Lamarck, though severe, are not altogether unjust, his suggestion that at the present time no experimental biologist regards the hypothesis as deserving of further consideration can come only from bias or from ignorance, or from both. For it is only, I think, of English-speaking biologists that this is even remotely true. The "pernicious superstition" is not without supporters of distinction in Germany; and in France and, I think, in most other countries it is still widely in favour.¹

(2) *Views of H. S. Jennings*

Even in America, the land of dogmatic negations founded upon naïve belief in the all-sufficiency of mechanistic principles, there is at least one biologist of high distinction who manifests an open mind towards the Lamarckian hypothesis. In his most interesting Princeton lectures of 1933 (5) Prof. H. S. Jennings carefully marshals the evidence which proves that Lamarckian transmission does occur in unicellular organisms. In some cases of exposure of a stock to changed environment

"a genetic change is induced, a variation that is carried on to later generations.... The difference in effect appears to be connected with the length of time that the effective agent continues to act. If the organisms are subjected for generation after generation successively to special conditions, so that these act for a very long time, then the constitution may become changed. There is produced an alteration in the inherited characteristics, a change that is passed on to later generations. This factor

¹ I well remember how after a lecture at the Harvard Medical School by the distinguished Dutch experimental physiologist, Lucien Kappers, I said to the lecturer: "Your whole lecture implies the reality of Lamarckian inheritance." And his answer was: "Yes! But why not?"

of time, or of something connected with the time, of action, appears of great interest. . . . In unicellular organisms such results have been produced, not by one or two external agents only, but by many different types of environmental conditions, by conditions that operate on the organisms in their normal life. Many different types of inherited alterations have thus been produced. Some of them are degenerative [like most mutations of multicellular organisms]. . . others are distinctly to the advantage of the organism. They are what may be called adaptive changes in the hereditary constitution (p. 103)". But he adds that "such a result. . . is not readily or easily reached: many investigators who have attempted to produce it have failed".

If such a result is not easily to be achieved experimentally in unicellular organisms, it may well be still more difficult to achieve in the multicellular. Nevertheless, Jennings concludes his long exposition of Lamarckian transmission in the unicellular organisms with a brief reference to my experiment, which seems to show on his part an open mind towards parallel effects in the multicellular organisms. He remarks of the latter: "One might say that all theory has been for it, all experience against it." I must confess that this dictum seems to me neatly to invert the true statement.

This epigrammatic statement accords ill with the following passage from Jennings' earlier book, *The Biological Basis of Human Nature* (New York, 1930):

The commonplace assertion is that the inheritance of acquired characters is disproved; that no competent investigator harbors the doctrine; that the matter is out of court, no more to be considered. Yet the recent and present technical literature of zoology abounds in experimental investigations devoted to this question. Eminent authorities, summing up the evidence, have affirmed that almost all experimentation directed on the matter supports the inheritance of acquired characters.

And it is one particular theory alone which stands in the way, the theory, namely, that the genes and the genes alone are directly concerned in hereditary transmission. This may well be described in Morgan's language as "a pernicious superstition", one which obsesses most of the geneticists.¹ The better of them will admit, commonly in a very brief paragraph or a footnote, that we have definite proof that something other than the genes (spoken of generally and comprehensively as the cytoplasm) plays an important role in heredity, and then, throughout the rest of their discourse, they speak as though they had completely forgotten this admission. Even Jennings illustrates in this way the

¹ If a superstition may be defined as a false belief used, in spite of accepted knowledge to the contrary, as a premise for reasoning for action, this is a true superstition, and it is a pernicious one; for it has led Morgan and other geneticists to pour cold water upon the Eugenic movement so greatly in need of their support.

power of this obsession. Further, most of them assume, implicitly or explicitly, that if and when all the distinguishable traits of any species shall have been correlated with various genes, as indispensable conditions of the development of those traits, the whole problem of heredity will have been solved, so far as that organism is concerned; or as they put it "the mechanism of heredity will have been solved". When Jennings writes: "Experimental genetic science has said of late with some justification that 'the problem of heredity is solved'", that is the implication of the phrase. They do not seem to realize that the demonstration that a certain array of genes are indispensable conditions of a corresponding array of 'characters' of the adult organism leaves all the dynamic problems of heredity untouched, the problem of how the infinitely complex marshalling and ordering of these indispensable materials is effected. Now if the conditions of heredity consisted wholly and solely in the presence of the genes (or also in their spatial distribution in the germ cell) it would be very difficult to conceive that parental activities could produce new genes or new specific arrangements of them in space. And this I submit is the real ground of the geneticists' bias against the Lamarckian hypothesis. It commonly remains implicit, but now and then is stated, as in the following comment with which Morgan in the article mentioned above (4) concludes some criticism of my earlier reports:

"If such acquirements are really inherited we must be prepared either to face complications or declare a miracle. However in the absence of any knowledge as to how such training could possibly affect the germ cells... this point need not be further discussed." And he goes on to say: "If such effects are transmitted, and especially if these are pre-eminently adaptive ones, there is every reason to suppose that evidence of such transmission should be obtainable—in fact should be abundantly apparent to anyone making careful observations. There is a lack of care in experimentation and of acumen in reasoning from the evidence manifest in most of the reported results. To suggest that the influence may not be felt unless extended over many or an indefinite number of generations removes the problem from the field of experimental science." [But only for those who have not the patience to persist. Compare here Jennings' demonstration that long continued exposure of many generations is necessary to produce genetic variation and manifest Lamarckian transmission in unicellular organisms.¹] He continues: "The situation is even worse when behavior

¹ Jennings writes: "The organism under the influence of the environment acquires powers to do certain things that it could not do before; to bring about chemical reactions of which it was before incapable, and so to live under conditions under which it could not formerly live. These acquired powers are inherited for many generations after removal from the conditions which induced them.... In producing such results, there are certain important relations between the length of time that the special environmental conditions have acted and the intensity and duration in generations of the genetic variation that it produces. In many cases the degree or intensity of the inherited effect is proportional to

patterns are considered, for all such adaptive reactions are highly complex, involving intricate nerve and muscle adjustments. It is, I venture to think, beyond the imaginative capacity of biologists familiar with what is best known to-day in embryonic development and in genetics, to suggest any rational process by which such complexities could be carried over *as a whole* from the body cells to the germ cells."

There we have it! The assumption so fatal in biology that only that which is explicable in terms of established physical and chemical principles can happen. But if we accept this assumption and set up "the imaginative capacity" of well-trained geneticists or of any other biologists as setting the limits of the possible, we shall have to deny heredity *in toto*: for it is certain that, in spite of the brilliant labours of Morgan and his fellow geneticists on the gene-map of *Drosophila*, no one to-day can imaginatively explain how, given these genetic materials, the genes and their spatial arrangement in the chromosomes, these materials, multiplying themselves again and again, become by any process (rational or irrational) built up into the differentiated and infinitely complex and specifically structured adult organism. And it is, I submit, undeniable that this would remain true, even though we should have ascertained the precise chemical composition of each gene and of all the constituent substances of the cytoplasm.¹

(3) *Criticism by J. B. S. Haldane*

Another eminent geneticist who shows on the whole an admirably open mind on our question, Prof. J. B. S. Haldane, is good enough to say that "it is clear that McDougall's experiment is in a class by itself" and that I have made out a *prima facie* case. Nevertheless, he at once shows the cloven hoof of bias by adding: "It is, moreover, worth noting that in another experiment, broken off after four generations, McDougall found that the time needed for training increased in successive generations. He put this down to an increase of timidity, *thus contriving to eat*

the length of time that the effective environmental condition acts. This is notably the case with the inherited damage resulting from living under bad conditions; the damage does not appreciably begin till the conditions have existed for many generations; it increases in amount as the bad conditions last longer, and finally may result in destruction of the stock. The constitution is progressively modified the longer the conditions last. The same relations are found in many cases in the production of inherited resistance by the action of heat or chemicals; the longer the action, the greater the resistance produced. The action of the environmental conditions on the materials of the organism is progressive...and the longer the environmental condition acts, the longer does the acquired character persist, i.e. the greater the number of generations through which it is inherited, after removal from the conditions that induced it" (*op. cit.* p. 115).

¹ A few vague references to 'physiological gradients' do not really fill the bill.

his cake and have it."¹ This caustic comment clearly refers to my experiment on training rats in a wheel to jump over a section of the floor which bore a shocking current, as described in my First Report. Now in that experiment I had looked to find the transmission of the mechanized habit of leaping across this section of the floor; but what I actually found was an increasing aversion from the shock, which had to be repeated a number of times in order to induce the leaping. The rats showed the increased aversion, not by leaping after fewer shocks, but by turning aside and clambering on the side-walls, thus avoiding the shock altogether. This result is in perfect harmony with the result of the tank experiment; in both cases the most obvious and striking change of behaviour manifested in the successive generations is an increasing or more readily induced anticipation and avoidance of the shock. It was this experiment that led me to devise the tank experiment: for, I argued, if here increasing aversion from the shock becomes fatal to my continuance of the wheel experiment, let me devise a different experiment designed to measure the degree of increase of this aversion. And just that is what my tank procedure does. Haldane's caustic and highly damaging comment is therefore entirely unwarranted and unfair, and, coming from so intelligent a critic, is, I submit, only to be explained as a product of adverse bias.

(4) *Experiments of W. E. Agar*

Prof. W. E. Agar of the University of Melbourne, after failing to find any evidence of Lamarckian transmission in an experiment involving a hundred generations of two species of copepods, has, like Crew, set out to repeat my experiment (6, 7). I have no complaint to make of his procedure or of his comments and criticisms on my work, all of which seem eminently fair and judicious. He reports on five generations only and finds no clear evidence of improvement of facility. My only comments here are that (1) the number of generations is small; (2) the task for some undefined reason seems decidedly easier than that set by my procedure and tank, requiring a decidedly smaller number of shocks before avoidance of the shock is acquired; (3) and this in spite of the fact that Agar's shock seems to be very decidedly weaker than the one used by me.

As to his criticisms on my procedure, I admit their validity, but I submit that they are all of minor importance. Chiefly they are as follows:

¹ Report of a lecture delivered before the Royal Institution on 22 April 1932, entitled "The Hereditary Transmission of Acquired Characters".

(1) that my shock may have been increased in intensity. I have already insisted that my method of regulating it provides against any but slight variations; that any increase of intensity can only be within very narrow limits, whereas large differences of strength of shock were found to give (in an experiment designed to throw light on this) relatively small differences of rate of learning. (2) "A progressive change in the constitution of the rats due to factors other than the inheritance of the effects of training." Agar insists that the only way to rule out this factor is to run parallel series of control batches of the same origin and same generations as the continuously trained series, in the way that Crew has done and Agar himself is doing. I do not question the value of such control series. And since my critics attach so much importance to controls of this kind, I may here remind them that I did run such a parallel series in my 16 generations of Bath rats, trained to escape from a water-maze without any experience of electric shocks. This parallel series was a feature of my original plan. And I attach considerable importance to the fact that a batch of 22 rats made up from 7 litters of the 16th generation of my Bath rats (which were of same generation and of same parentage as my Tank group, the Tank and the Bath series being derived from the same small group of Wistar rats by splitting the litters born to them equally between the two groups, and, but for the different training in each generation, submitted to identical treatment in the same room) made on the average more than 107 errors per rat with range from 69 to 129+ (cp. Table VIII, this *Journal*, xxviii, 326).

Unfortunately at that date I still continued the lazy practice of not completing the training of the slower rats; and out of the 22 rats 16 had not begun to show any clear signs of learning at the time when the training ceased. It is certain that these 16 rats would between them have made many more errors, which would have brought the figure for the average error of the group far above the 107 average of errors actually made. It is certain also that, if the training had been completed, the number of errors that would have been made by the slowest rat of this batch would have been far above the figure 129 which was the highest number of errors actually made. I add that 41 rats of this same generation of Bath controls were subjected to the zero-day test of six immersions with alternating light but no shock, and they made 121 turns to the dim gangway, *D*, and 125 to the bright, *B*. I greatly regret that this series of controls was not continued.

It is one of the difficulties of this experiment that a mistake of this kind, once made, can never be retrieved except by beginning the whole

laborious business over again. But here we have a control group of 22 rats fulfilling all the most rigorous requirements, except that the number of its members might with advantage have been larger. Comparing them with the 23 Tank rats of the parallel generation, namely T.R. 13, we see that these show decidedly superior facility in respect of average error, and also in respect of the number of errors made by quickest and slowest rats of the two groups. I have had to write the average error for the T.R. 13 group as 68+. But as all but six rats of the 23 had completed their training (as contrasted with 16 of the group of 22 Bath controls) it is very improbable that the figure would have been much higher if training had been completed.

I add one comment upon a feature of Agar's discussion which seems to me to imply a mistaken interpretation of the conditions of learning. Again and again he writes of *learning capacity* in a way which implies that in his mind the words stand for some factor distinct from all the others which may play a part in the mastery of the task, such as aversion from the bright gangway. He would seem to have in mind something of the nature of Spearman's *G* factor, and to hold that the question at issue is essentially whether this factor is improved or increased by the training process in successive generations. I object, first that there is no clear evidence of any such factor in rats¹; secondly, that the improvement or the learning of the individual rat in course of training in the tank depends upon a number of different factors, but largely and essentially upon a gradually increasing aversion from the bright gangway. Agar himself describes just such evidence of this as I have cited above. All my observations go to support the view that it is in respect of this factor (the rate of acquiring an effective degree of aversion from *B*) that the superiority of my later generations of trained stock largely or perhaps wholly consists; further, that in this gradual process of acquiring such a degree of aversion from *B* as will force them to take *D*, they start at a point which their ancestors and all controls attain only after receiving a considerable number of shocks on *B*. This capacity to acquire rapidly, i.e. after a few shocks only, this effective degree of aversion from *B*, is of the essence of superior *learning capacity* if that term be used in the comprehensive sense in which alone, in the present state of our ignorance, we are justified in using it.

¹ I remind that one of my students, Dr T. L. McCulloch, has made a careful search for such a factor and failed to find any indication of it(8, 9).

(5) *Criticisms by T. M. Sonneborn*

The criticisms made by Dr T. M. Sonneborn⁽¹⁰⁾ have in the main been answered in the foregoing paragraphs of this report and in my Third Report⁽²⁾. They concern chiefly (1) the possibility of unwitting favourable selection, which, I think, is sufficiently answered by my W.H. series; (2) the possibility of the apparent improvement being due to increase of strength of shock. Sonneborn takes Rhine's figures obtained with very small batches of rats and large differences of strength of shock; and suggests that the differences of facility shown in my tables are consistent with this suggestion. Even the differences of my Second Report could be thus dealt with by Sonneborn only by some stretching of probabilities, and by ignoring completely the evidence of my control groups. I submit that the much larger differences of the third and present reports go far beyond any differences which can be attributed to differences of strength of shock with any appreciable degree of probability. My method of regulating the strength of the current is really very delicate; a very small change of strength makes the difference between tetanizing all the rats every time and allowing most of them to pass without tetanization. In my Second Report⁽³⁾ I stated that the strength of my current certainly did not vary more widely than between the strength of Rhine's medium current and his strong current. Sonneborn interprets this as an admission that it did vary as widely as this difference, which it was not meant to be. I believe any variations of my strength of current have actually fallen within much narrower limits. I admit that more work upon the effects of different strengths of current is desirable. Yet the evidence on this matter as it stands is as follows: A heavy shock caused learning with an average of 27.8 errors, while a much lighter shock (the medium shock of Rhine's experiment on varied strength of shock (⁽³⁾, p. 212)) caused learning with an average of 54 errors. On the other hand, with shocks which varied certainly not more widely (and probably much less widely), my T.R. series (Table VII, this *Journal*, xxviii, 323) range in respect of average errors per rat from 8 to 80; and if the evidence of the control groups be taken into the account, as it logically must be, that range is increased from 8 to 130. Further, my W.H. series (Table X, *loc. cit.* p. 331) ranges from 27 to 144, or, going back to the ancestral group of this series, to 215.

The future of this experiment is uncertain. I am an old man and a cripple, and cannot foresee the duration of the opportunities for prosecution of it which I still enjoy at Duke University.

POSTSCRIPT

I add here two points of some importance which I was too stupid to make in the writing of the foregoing article. In Part II of this article I pointed out two principal defects of Crew's procedure, suggesting that these are the principal grounds of Crew's failure to discover any increase of facility in his 'experimental series'. I now point to a third feature of Crew's procedure which may have, and, I think, very probably has, played a similar role; namely, his postponement of the beginning of actual training with the shock to the 75th day after birth. Whereas the training of my rats was begun in the great majority at or about the 30th day after birth (occasionally in case of large and vigorous animals a few days earlier, and in a few cases somewhat later), Crew began a preliminary investigation of each rat on the 57th day and continued this through 17 or more days.

Now I have throughout acted upon the assumption that the earlier the age at which training begins, the more likely is it that Lamarckian transmission of its effects may ensue. This assumption is founded (somewhat insecurely no doubt) on the truth (generally accepted not only by Jesuits and psychoanalysts but by all men) that training of children at an early age produces far more lasting and profound effects than any training at later ages. Crew's rats at the beginning of their training with the shock are comparable to adolescent humans (say 16 years of age) while mine are comparable to infants of about two years of age, just beginning to run about freely.

My second point is a suggestion in explanation of the fact that the B.H. series shows no more rapid improvement than the W.H. series. This seeming anomaly has been the occasion of much anxiety to me, and it is only now in correcting proof that I see what seems to me the explanation of it: namely, the evidence points more and more clearly to the view that the transmission of the aversion from the bright gangway is a function of the number of shocks received. In each generation of the W.H. series, the parents of the succeeding generation are the 50 % which have had the most shocks; whereas in the B.H. series the opposite is true, the parents of each generation are the 50 % which have had fewest shocks. And the difference in number is very large in each generation. Thus, I find that, of the later generations of the two series, the parents of the W.H. groups have received on the average rather more than three times as many shocks as the parents of the corresponding B.H. groups; the last 42 W.H. parents received an average of 38 shocks each, while the last 41 B.H. parents received an average of only 12 shocks each.

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HYPNOSIS, SUGGESTIBILITY AND PROGRESSIVE RELAXATION¹

BY WILLIAM BROWN

From the Institute of Experimental Psychology, Oxford University

- I. *Introduction and historical account* (pp. 396–399).
- II. *Theory and practice of hypnotism* (pp. 400–401).
- III. *Therapeutic hypnotism with relaxation* (pp. 401–402).
- IV. *Hypnosis and progressive relaxation: experimental evidence* (pp. 402–406).
- V. *Hypnosis and the will* (pp. 406–410).
- VI. *Hypnosis, analysis and transference* (p. 410).
- VII. *The psychology of leadership* (pp. 410–411).
- References* (p. 411).

I. INTRODUCTION AND HISTORICAL ACCOUNT

THIS paper, like ancient Gaul, will be divided into three parts—historical, experimental and clinical, and therapeutic. I emphasize the third part, because, as I speak of the first and second, it may be considered by some that this is a very questionable subject, or even a dangerous and harmful one, and that the sort of things that are done in hypnotism are not of a nature which will help mankind. But in the third part I shall be dealing with the constructive side, and the way in which the knowledge of this subject may be used therapeutically for the help of the individual.

The history of hypnotism in modern times begins with the work of Franz Anton Mesmer, who flourished in the eighteenth century. He went from Germany to Paris in 1778, and for a short time he was all the rage in the French capital, until the Academy of Sciences appointed a Commission (of which Benjamin Franklin was a member) to investigate his methods and claims. That Commission reported unfavourably, and Mesmer went back to Germany shaking the dust of Paris from his feet.

Mesmer did not induce hypnosis as we understand it now. He grouped his patients in a large room around a tub (the famous *baquet*)

¹ A Lecture delivered at a meeting of the British Psychological Society, held at the London School of Hygiene and Tropical Medicine, Gower Street, on Thursday, 29 April 1937; and repeated, in shortened form, before Section J (Psychology) of the British Association at Nottingham on 3 September 1937.

in which were powdered glass, iron filings, and water, and through the lid of the tub there protruded jointed iron rods. Each patient would take hold of one of these rods, place it against the part of the body where he was suffering, and after a suitable and impressive interval, Mesmer, clad in a lilac-coloured dressing gown, would enter the darkened room to soft music, and slowly proceed around the group, touching one after another. Some of those whom he touched fell into hysterical attacks and afterwards pronounced themselves better. Mesmer believed that it was a real physical influence which was exerted, the same as that which was supposed to enable the planets to act upon the earth and influence human destinies. The French Commission held that it was all to be explained in terms of the imagination of the subject. If the subject thought he was near something which had a magnetic attraction for him he would fall into convulsions. Although 150 years old, the report of the Commission is very much up-to-date. It explains Mesmer's results in terms of the activity of the imagination of the subject.

In the same year—1784—in which that report was published the Marquis de Puységur discovered the state of artificial somnambulism. He was mesmerizing a shepherd boy named Victor, and found that the youth went into a sleepy state, and that in such a state he walked about for some time afterwards. The result of this work was to arouse great interest in France in the study of *extra-lucidité*, although all dealings with it were frowned upon by the medical faculty. One Paris physician, for example, would journey into the country, ostensibly to pursue botanical studies, but actually in order to investigate a somnambulist subject lying asleep in a cottage.

Towards the beginning of the nineteenth century we find the Abbé Faria and others at work on these phenomena. Much of this work was re-discovered and re-presented by James Braid, a Manchester physician, in 1843. It was he who invented the term 'hypnosis'. A French magnetizer had been demonstrating in Manchester, and on testing his subject Braid came to the conclusion that the phenomena were genuine. He found that he could produce them by getting the patient to stare at a cork tied in the middle of his forehead; later on he used his bright lancet case. Braid was the first to emphasize the difference between hypnotic slumber and ordinary sleep, which other writers have confused. From thirty to forty years later both Liébeault and Bernheim considered that the sleep of the hypnotized patient was the same as normal sleep, but they had not experimented in the scientific sense, and they were working as clinicians rather than as scientific psychologists.

Towards the end of the nineteenth century a great conflict arose between the Charcot school at the Salpêtrière and the Bernheim school at Nancy. Charcot, a physiologist and neurologist, thought he could investigate the hypnotic state in a purely scientific and objective way by the method employed in the investigation of organic neurological cases. He spent a great deal of time over the case of three young hypnotized girls; he did not himself hypnotize them, and he was to a certain extent the unintended dupe of his resident physicians. Charcot thought he could distinguish three stages of hypnosis: the stage of lethargy, of catalepsy, and of artificial somnambulism. In the first stage the eyes are closed, then, on the eyes being forcibly opened, the subject passes into a state of catalepsy with rigidity of the limbs, and later, if the top of the head be rubbed slowly, into artificial somnambulism, in which he can walk about and perform certain movements while still in the hypnotic state. Charcot thought that a magnet had an influence on these patients, shifting their symptoms from one side of the body to the other, and named the phenomenon 'transference'—a term used in psychiatry in quite a different sense to-day.

Bernheim at Nancy was convinced that all these results were due to the activity of the imagination of the patient. His theory of hypnosis was that it signified a state of artificially increased suggestibility, and that this could explain all the various phenomena I shall presently enumerate. On the other hand, Charcot, besides making the mistake of supposing that what was true of the trained subjects with which he worked, and in which the various stages were repeated over and over again in a particular order, was true of patients in general, considered the state of hypnosis to be a state of artificial hysteria, hysteria meaning for him a functional nervous disease with no obvious organic basis, but characterized by a particular kind of dissociation. Prof. Pierre Janet⁽¹⁾, who worked under Charcot at the Salpêtrière, has set out the history of this controversy in his great book *Psychological Healing*.

If now I may be autobiographical to some extent, I would state that my experience with the practice of hypnotism began twenty-four years ago when I saw at King's College Hospital a case of amnesia or loss of memory extending over three years. By practising the hypnotic method I made the patient fall into a trance, and in that trance, at a suggestion from me, his memory was restored (⁽²⁾, pp. 63-8). Later, during the war, I was in charge of the nerve cases in the Fourth Army on the Somme, and afterwards those of the Third and Fifth Armies, and I saw something like 5000 cases during the war period. I tested over 15 % of

these with hypnotic methods. They had suffered from amnesia, and there were other symptoms such as functional paralysis of the legs, loss of voice, loss of control in various directions, with tremors. On hypnotizing these patients—and they were very easily hypnotized—if it was suggested to them that their lost memories would come back they came back practically at once, and with the return of the memories the other symptoms tended to disappear. The more extensive the amnesia and the dissociation, the more easy was the patient to hypnotize, and the more rapid the restoration of the memory. I came to the conclusion on the basis of these experiences that the hypnotic state could be produced especially easily in this type of dissociation. This corresponded with the generalization Charcot had made that hypnotism was an artificial hysteria, though one could not accept other parts of Charcot's theory, which had been thoroughly disproved by the Nancy school.

The Bernheim theory of artificially increased suggestibility is also well founded, but Bernheim under-estimated the importance of the fact of dissociation, and when we come to generalize about hypnosis later on we must bear that in mind. I have not changed my view at all on this matter. I found that my results were in harmony with those which Pierre Janet had been getting before the war. I met him at the International Congress of Psychology in Oxford after the war (1923) and we compared notes on the subject. We were entirely agreed that the dissociated patient was easily hypnotized. But if one emphasizes increased suggestibility as the fundamental characteristic of the hypnotic state, one is overlooking the fact of dissociation, because suggestibility can be increased in all sorts of ways independently of the kind of dissociation of which I am speaking.

After Bernheim's death M. Emile Coué⁽³⁾, a pharmacist at Nancy, continued his clinical work and practised a method of auto-suggestion involving hypnotism. The examples he used to illustrate the process of auto-suggestion and the working of what is called "the law of reversed effort"¹ are clear-cut examples of dissociation and the hypnotic state. One of his demonstrations consisted in getting the subject to clasp his hands tightly and more tightly and afterwards to try to unclasp them, while at the same time thinking intensely, "I cannot separate them. . . I cannot." In that case the hands, instead of separating, become more closely linked. That is a state of psychoneurosis, an abnormal state.

¹ "When the will and the imagination are in conflict the imagination always wins."

II. THEORY AND PRACTICE OF HYPNOTISM

Similarly a subject may be asked to lie on a couch with a head-light behind him, while a bright mirror—I have used an ordinary ophthalmoscope mirror—is held about 10 in. from his eyes, slightly above the horizontal plane of vision, so that he has to converge his eyes slightly inwards and upwards when a beam of light is reflected into them. As he gazes in that way it is suggested to him that he is becoming more and more drowsy, that his eyelids are becoming heavier and heavier with sleep, that in a moment his eyes will be closed, that he will soon be asleep, that on the count of three he will be unable to open his eyes—and in a large proportion of subjects it is then found that they cannot open them. The more they try the less they succeed in doing so.

Just after the war a patient came to me wishing for treatment, but specially requested me not to hypnotize him. He lay down on the couch and quite accidentally closed his eyes and found that he could not open them, although I had not spoken a word. He became annoyed, indeed very angry. He had hypnotized himself, he had got into a state where the law of reversed effort was at work, he was in a dissociated state, a “house divided against itself”.

It is never normal when a person finds himself unable to do a thing which he customarily can do. Whatever people may say about the normality of hypnosis, it is abnormal if a man cannot open his eyes or unclasp his hands or drop his uplifted arm. Hallucinations, positive or negative, are abnormal. A deeply hypnotized subject may be told that you are going to show him your photograph, whereupon, on being presented with a blank sheet of paper, he can see your photograph on it. That is a positive hallucination. He can be sent to sleep and it may then be suggested to him that on awakening he will be able to see only five people in the room, although in fact there are six present. He may be told that a certain person has left the room, although in fact he has not, and then on opening his eyes he will count the others and studiously avoid looking at that particular person. That is an example of negative hallucination.

All these are ordinary recognized symptoms of the hypnotic state. The method points to one thing, namely, dissociation, the setting of the mind against itself, the passing over of the control of his imagination from the patient to the hypnotist. It must be abnormal even if the patient himself is normal to start with—and I readily admit that a

certain proportion of quite normal people can be put into a mild hypnotic state, and some into a deeper hypnotic state.

That is hypnosis, as generally described in terms of inability to do certain things. If the hypnotist wishes to show off his powers, he can demonstrate to an audience that a person is really hypnotized by suggesting to him that he cannot open his eyes or that, for instance, having a glass in his hand, he cannot drop it or cannot hold it. The patient's imagination is under the control of what the hypnotist is saying. In other words, he is in a dissociated condition, the full power over his eyes or his hand being dissociated from the rest of his mind.

III. THERAPEUTIC HYPNOTISM WITH RELAXATION

But there is another side to the story. So far as I have gone I have said nothing about relaxation. That, indeed, would not come into a mere demonstration of hypnosis, but it is of very great importance when hypnotism is used in treatment. In all my various books in describing the process of hypnosis I have emphasized relaxation.

In the therapeutic method the patient lies on a couch, not necessarily looking at a bright object, and is asked to close his eyes, to give himself up to sleep, to let all his muscles go limp. He is told to breathe slowly and rhythmically, and in so doing his muscles progressively relax. Relaxation begins with the small muscles of the hands and feet, extends up the arms and legs, over the body, and includes the head, neck and face. I emphasize the importance of relaxation of the muscles of the back of the neck and the muscles of the face, and, especially, among the facial muscles, those that have to do with eye and eyelid movements. That is the part of relaxation which is of very special importance. There is here no question of setting one part of the mind against another. The more relaxed the subject the more drowsy he becomes. As muscular tension disappears it is followed by lessened tension of the nerves and the mind, and in this state suggestions can be given with beneficial effect. The suggestions should always be of a positive, not a negative nature. They should never suggest inability or lack of power, but always be in the direction of increased power, increased vitality, and enhancement of the normal in every respect. The patient is made so to lie for an hour at a time. I have screens closed around him, and every few minutes I give him suggestions of a special and of a general nature, then close the screens again and leave him to go on relaxing. After a few hours of treatment on different occasions he will become very relaxed indeed, and the suggestions he receives will have more and more effect.

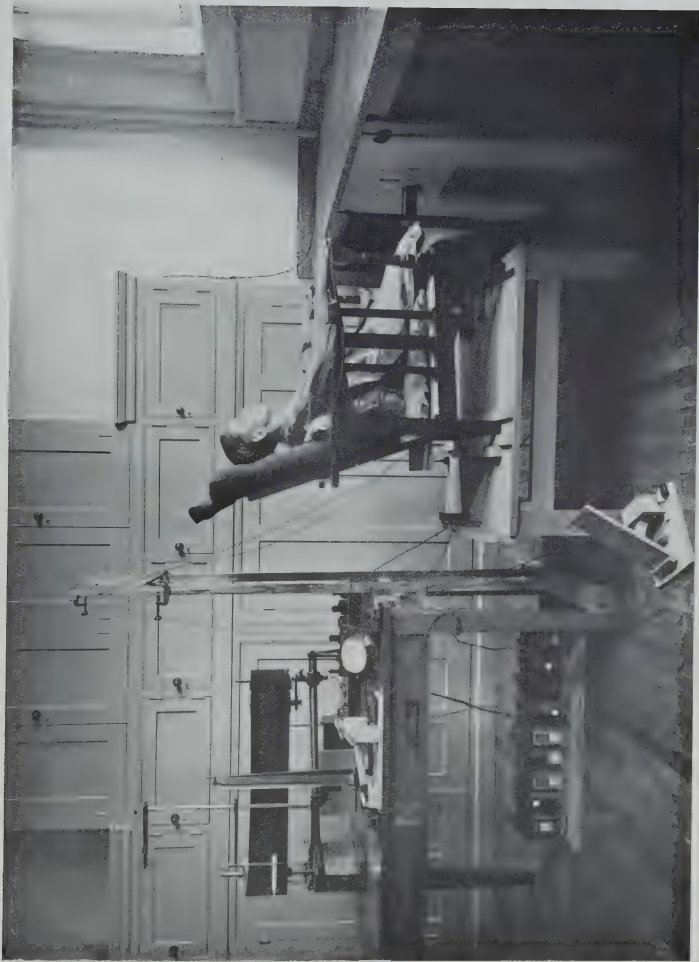
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Are these people hypnotized by such methods? One certainly finds that they benefit, and in a proportion of cases the suggestions take strong effect. They are themselves able, moreover, to use the method of auto-suggestion, practising it last thing at night and first thing in the morning. Suggestions may be given them about the background of their mental condition. They may be given general suggestions that when they wake in the morning it will be after refreshing sleep and with a new physical fitness. Any modern psychotherapist in such cases would also carry out a certain amount of preliminary analysis, finding the general underlying psychological reasons for the condition. But there are some cases in which much analysis cannot be done.

IV. HYPNOSIS AND PROGRESSIVE RELAXATION: EXPERIMENTAL EVIDENCE

Does the benefit follow from the hypnotic state or from the state of relaxation? Here we come to the experimental work. A great deal of experimental work has been done on hypnosis, especially in America. One general measure of increased suggestibility is obtained by recording on a smoked drum in the form of a curve of oscillation the responses of a blindfolded subject to certain suggestions, as, for example, that he is moving forward. Another method we have found useful is to note the change in the knee-jerk. M. J. Bass⁽⁴⁾ in America originally carried out some experiments in this direction, and those have been repeated by myself and my colleague Dr W. Stephenson at the Institute of Experimental Psychology at Oxford. A kymograph is used with a long sheet of smoked paper and a lever to record the curve. The subject sits in a kind of dentist's chair and a hammer is employed to give every 10 sec. a smart blow to his patellar tendon, so as to bring out the knee-jerk as fully as possible. By a thread from the subject's heel passed round a pulley, a tracing of the knee-jerk is reproduced on the smoked paper. Bass found, using similar apparatus, that a subject who had really gone off to sleep lost his knee-jerks, but if put into a hypnotic slumber the knee-jerks remained and were, in fact, as pronounced in the hypnotic as in the normal state, if not more so. He suggested that one method of distinguishing normal sleep from hypnotic sleep was the effect on the knee-jerks. In normal sleep the knee-jerk disappears, in hypnotic sleep it remains.

Another criterion distinguishing hypnotic sleep from normal sleep in Bass's experiments was that, if suggestions are given to a subject before he passes into normal sleep that he will at a given signal, for example,



Showing solenoid hammer (right), subject's chair, and recording apparatus (left).

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a faint buzzing sound, press a button, he will fail to do so, but if hypnotized, although he may seem to be in a very sleepy condition, he will be sure to press the button whenever he hears the buzzer or whatever the signal may be. Thus we have two discriminating marks of hypnosis as contrasted with normal sleep.

In carrying out experiments, combining with the method of hypnosis the suggestion of relaxation, my method has been to get the patient to fixate a bright light and then to get him to relax his muscles. I suggest to him that he is becoming drowsier and drowsier, that he should fix his mind on the idea of sleep, emptying his mind of all else, that he should

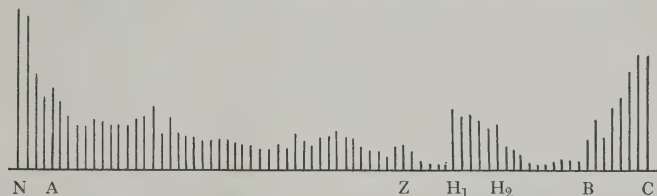


Fig. 1. Showing knee-jerk measurements during the course of progressive relaxation, suggestion, and hypnoidal state. Subject J. C. Age 22 years. Made on Feb. 13, 1937. Each vertical line is the average of four consecutive measurements of the knee-jerk, which is stimulated at 10 sec. intervals. *N*, normal knee-jerk, wide awake condition of subject. *A*, suggestion is given: "Go to sleep." The suggestion is repeated at intervals between *A* and *Z*. *Z*, development of hypnoidal state. Subject is in hypnoidal state from *Z* to *H*₁. Quiet arm movements can be made, without changing the relaxed condition of the knee-jerk. *H*₁, right arm catalepsy; strong suggestion that subject cannot drop his arm no matter how hard he tries. Subject is unable to drop his arm, in spite of apparent effort to do so. Note the reinforced condition of the knee-jerk. Between *H*₁ and *H*₂ left hand catalepsy was also demonstrated. *H*₂, return to relaxation with suggestion. *B* to *C*, gradual return to waking state, and normal knee-jerks.¹

breathe slowly, deeply and regularly, and let all his muscles relax. Thereupon the patient becomes almost completely relaxed with knee-jerks abolished though not yet asleep. In such a condition I may give the suggestion that, after I have counted three, he will not be able to open his eyes, and at once the knee-jerk, which had disappeared, returns. One may link up the patient with an apparatus for testing the psychogalvanic reaction, finding a resistance in the hand of 20,000 to 30,000 ohms on starting, which resistance rapidly rises as he becomes more and more drowsy and relaxed. Conversely, directly hypnotic experiments begin with the same subject, the resistance falls.

¹ I wish to express my grateful thanks to Dr W. Stephenson, the Assistant-Director of the Institute, for the preparation of Plate I and Fig. 1. W. B.

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One has found that thoroughly normal undergraduates respond readily to these simple suggestions given in a hypnotic procedure. At the particular suggestion they become unable to open their eyes, their arm becomes rigid, or they are unable to drop an object or to hold it, as the case may be. But as one watches such experiments, one feels that they cannot be good for the subject. On one or two occasions they may be without harm, but when repeated again and again they must be detrimental to the subject. I have deliberately avoided such repetition, for subjects become quickly habituated to the condition. It is a habit phenomenon, but to produce an inability at another's word of command is not a habit to be encouraged. Let me add at this point that almost anyone, with a sufficient amount of care, deliberation and determination, can hypnotize himself. He has to narrow down his mind as it were to one visual point on the horizon, practise an absolute fixity, and he will feel rigid or limp as the case may be, and lose all other sensation.

On the other hand, the best subject we have treated in our recent experiments at Oxford was a definitely hysterical individual (not an undergraduate). He was a man who had received a shock as the result of a mishap at his employment (nearly falling from a ladder), and a day or two later he began a stammering habit. He responded extremely readily to the hypnotic procedure. I suggested to him that 5 min. after he woke up he would go to the door of the laboratory and turn off the electric switch; he did so, and when asked why he did it he gave an *ad hoc* reply, namely, that he generally turned off switches when he noticed them as, e.g. with his wireless at home—an example of rationalization. There seems to be a dissociated tendency which works on its own in response to suggestion at the right moment, and remains latent until that moment for manifestation.

The wide possibilities of experiments of this nature will be at once appreciated. There are hardly any limits to the amount of variation which can be applied. In these normal subjects on which we have been working the knee-jerk disappears with the suggestion of relaxation and returns at once on any hypnotic suggestion. The impression one gets is that the state of lethargy is a mixture of sleep and hypnosis. That is the view behind Prof. Clark Hull's *Hypnosis and Suggestibility* (5)—a book containing a very great amount of material derived from experimental work. Instead of regarding the first stage of hypnosis as just a hypnotic state, it should be regarded as a state of sleep with the hypnotic state intermingled. The patient is on the way to sleep, and so far as sleep occurs it is normal sleep, but whenever any suggestion is given in regard

to hypnotic procedure, the hypnotic state is immediately manifested. The sleep state is passive whereas the hypnotic state is active, and we come back to our problem of what we are to make of the state of relaxation which we can evoke by hypnotic methods.

The experimental work seems to indicate that the hypnotic procedure can produce relaxation, although, if relaxation be not mentioned, the hypnotic state nevertheless is produced and the various stages of hypnosis observed. If the same hypnotic method be used with suggestion, and relaxation be suggested, relaxation is produced very much more rapidly than in any other way. The method of progressive relaxation which I have described is similar to that which has recently been elaborated by Dr Edmund Jacobson⁽⁶⁾ in a work published under this title by the Chicago University Press in 1929. Jacobson enumerates thirty-two points of difference between relaxation and suggestion (⁽⁶⁾, pp. 303-8). It is true that relaxation as such, like sleep itself, is not a state of hypnosis. Jacobson's own method is first to get the patient to contract the biceps muscle against slight resistance, then to contract the triceps, the quadriceps, and the various other muscles of the body, thus becoming acquainted with the sensation of muscular contraction in these parts, and then to proceed by allowing that contraction to disappear and "go in the negative direction", thereby obtaining a state of relaxation. The patient is urged to relax still further to get rid of "residual tension". Jacobson finds patients get on better with relaxation when left to themselves, after some preliminary instruction. Dr Jacobson's book should be of course consulted for an adequate account of his method.

But I have found that, using the hypnotic procedure with suggestion, relaxation is produced very much more quickly, and the patient himself can produce very marked relaxation by self-suggestion. I regard this as part of the hypnotic state, the result of a hypnotic procedure, but in that procedure it is a positive normal achievement which is being suggested to the patient, not something abnormal, not a disability, not a crippling of his powers. The relaxation is progressive in that it starts with the muscles of the hands and feet, the arms and legs, and embraces all the muscles of the body, especially those of the back of the neck and of the face and round the eyes. From the voluntary muscles the process spreads to the involuntary, the muscles of the stomach and intestines, and probably those of the blood vessels, the autonomic nervous system is affected, and thereby still greater relaxation is produced.

In such a state suggestions have a much more powerful effect, and the suggestions should for therapeutic purposes always be positive, not

negative. One should not suggest a disability. It must be weakening to a person to have his will taken from him by someone else, and to be told that he cannot open his eyes or unclench his hand. On the other hand, it is not weakening to his will if, relaxing more than he would otherwise be able to do, he receives in that state suggestions of greater ability in various directions. In that way he is helped to help himself, and instead of being made more and more automatic, as in ordinary crude hypnotic procedure, so that he is a danger to himself and to others, he wins through to greater power and achievement.

I had one case of a good hypnotic subject who, while in a barber's chair, gazing at a bright light just above him, went into a hypnotic trance and in that state walked out into the country for many miles. Although he had a certain control over himself, nevertheless he was a real danger. But in this method of constructive suggestion, what is being aimed at is increased power, increased strength of will, increased control.

V. HYPNOSIS AND THE WILL

There have been various definitions of the will. Behaviourists regard willing as a symbolic act effecting through a verbal mechanism a real act. They quote a very interesting experiment carried out by C. V. Hudgins⁽⁷⁾ in 1933. He was experimenting with a subject to whom he said "Contract", a moment later flashing a bright light into the subject's eye. As a result of the light, of course, the pupil of the eye contracted. He kept repeating the word "Contract", accompanied by the flashing of the bright light, with the result that eventually what is called a conditioned reflex was set up, so that presently when he said "Contract", but did not flash the bright light, the pupil still contracted. Then he went further, and instead of himself saying "Contract", got the subject to say it himself, and at the end of the experiment it was found that whenever the subject said or even thought "Contract", his pupil contracted, without the flashing of any light. Thus the subject was able to contract his pupil at will, a thing ordinarily impossible, by having built up a conditioned reflex through the motor mechanism of speech.

This is considered by the behaviourists a good illustration of the way in which will can be explained, but it is not adequate, of course, to anyone who has made a special study of the will-act. In the case of hypnosis, the hypnotic methods of producing results that ordinarily cannot be produced at will may be regarded as the production of a conditioned reflex. A conditioned reflex may be built up with a view to improving the digestion, so that the process of thinking that the digestive

functions are working energetically and healthily may, in a sub-verbal way, produce the desired effect. In other words, we can increase the scope of our will over the functions of our body. Ordinarily we say that the will can control the voluntary musculature, but not the involuntary, this being controlled by its own proprioceptive and autonomic processes, but by associating with these processes those which are under the control of the will we may get the others to some degree under the control of the will also, just as the subject in the experiment I have described acquired the power, not ordinarily available, of making the pupil of his eye contract at will.

It is my belief, on the basis of clinical evidence of cases which I have treated for many years, that this method of using suggestion treatment when the patient is lying relaxed and passive is a method of building up new conditioned reflexes, the results of which become more and more pronounced. We encourage the patient to think the words to himself, and thus he uses his conditioned reflex to increase the scope of his will power over his body. Is that fallacious reasoning? The more we meditate upon such an experiment as that regarding pupillary contraction the more impressed we shall be by the extent to which the theory of the conditioned reflex may explain the suggestion process.

It was thought at first that in the state of hypnosis a conditioned reflex could not be built up because of the inhibitory processes in the cerebral cortex of the person hypnotized. But H. D. Scott⁽⁸⁾ in America arranged an experiment whereby the subject at the sound of a buzzer received a slight electric shock in the finger, which, having been pressed against a plate, moved away from it at the instant of the shock, and the movement was registered on a travelling smoked paper. This was repeated a number of times with a person in a waking condition, and a conditioned reflex was built up so that later when the buzzer sounded, though no electric shock was given, the finger moved away from the plate, and sometimes the subject had a hallucinatory experience of electric shock.

On taking a number of subjects, half of whom were hypnotized and half not, and comparing them, Scott found that the group of hypnotized subjects built up the conditioned reflex more quickly and firmly than the others, so that a conditioned reflex can be built up, and even better built up, in a state of hypnosis.

The advantage of relaxation, I consider, is to prevent the morbid manifestations of hypnosis, the feeling of inability to do this, that and the other, the tendency to catalepsy, and all those tendencies which are disturbing to the onlooker, if not to the subject. There is a distinct danger

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in hypnotic experiments in dealing with adolescents, that one or two of them might have a tendency to schizophrenia, which might become apparent in the course of the experiments or subsequently. Hypnotic experiments, however, will not make a normal person a schizophrenic, nor will they be in themselves injurious to any one having the tendency to schizophrenia.

If, therefore, we face the question whether the hypnotic state is morbid or in itself normal, and, secondly, whether only abnormal people, or a particular type of abnormal people such as psychoneurotics, are hypnotizable, we have to say, I think, first of all, that everyone, normal or abnormal, is hypnotizable to a certain extent, different people to different degrees. Those who are easily hypnotized and hypnotized to a pronounced extent reach a state of artificial somnambulism. They have a tendency towards, if not a complete development of, psychoneurosis of a hysterical nature. Secondly, the manifestations produced in hypnosis are partly normal and partly abnormal. Those based on the suggestion of an inability to perform acts or on a conflict of the mind within itself or on stress arising between the imagination of the subject and his will are abnormal, and the more often they are repeated the more abnormal the person may become. Such a person is not helped by the hypnotic procedure, but an injury is being done to him. Using the same general method of hypnosis, however, if profound relaxation be suggested, followed by suggestions of greater power in various directions, the result is a movement in the normal direction. I find support for this view in the fact that cases can be cited in which individual powers were raised above the normal. The state of hypnosis, in itself, is neither morbid nor injurious.

Perhaps I may give two personal illustrations in support of this theory. In one of our big Public Schools a house-master, now retired, used to send his backward boys to me for suggestion treatment. I used to give suggestion treatment in the stage of muscular relaxation, and it so happened—of course, it might have been *post hoc, ergo propter hoc*—that subsequently these boys got through their ‘trials’.

The second instance relates to a great composer, now dead, who came to me on four separate occasions, saying that he feared his powers of composition were failing. I submitted him to a certain amount of analysis, or talking out, and then applied suggestion treatment, and with the method of relaxation his powers came back after a short course on each occasion. I am quite prepared to have it said that it was a coincidence, but the composer himself had a feeling of benefit, even after the

first occasion, and quite unmistakably after the fourth. The mechanism was no doubt that of removing inhibitions and mental conflicts. The same method can increase the powers of memory, and increase them permanently, not just for the moment. The patient can acquire a stronger memory altogether, the reason being that the method of relaxation and suggestion—the two processes together—removes inhibitions and doubts and the fear of not being able to remember, also reinforcing the power of imagination of success, which is so extraordinarily important. Anyone of strong will who can imagine success and hold that imagination firmly before him is exercising an essential process of will, a process of auto-suggestion, which I look upon as a training of the will, working through the normal to the super-normal.

Again, as I have suggested, the method may confer increased vigour in regions which are not ordinarily under the control of the will, such as the digestive and excretory processes. It may assist ordinary medical treatment and presently render it superfluous. In a case of constipation, for example, suggestion treatment might be combined at first with the giving of the appropriate medicine, and then, while the suggestion was continued, the medicine given might be diminished in quantity or strength until coloured water was substituted, or nothing at all was given. A very good way to help to get normal function is to start with a certain amount of help from outside in the form of recognized drugs, and at the same time to imagine the peristaltic movements of the intestine becoming more and more vigorous, and presently, when the medicine has been given up, the suggestion can continue its work in the full restoration of normal function. One should try to imagine as vividly as possible the peristaltic movement taking place and say, "I have got control not only of my voluntary muscles but of my stomach and intestines", and although that method alone, without external help, may not avail, yet, the external help having been afforded—having, so to speak, given the boat the push-off from the shore—the suggestion may become completely effective, in the form of a new conditioned reflex or set of reflexes.

Perhaps I should add that it is the general experience that the less fatigued a person, the more easy it is to produce the hypnotic state, because such a person is better able to concentrate. As we well know, in ordinary sleep the very fatigued person does not sleep as easily as one whose fatigue is only moderate. Alcohol does not seem to increase hypnotizability to any extent (although alcoholism does), but experimental evidence has shown that scopolamine is capable of so doing. Idiopathic epilepsy is not responsive to hypnotic procedure, nor is

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petit mal. Any response is in the direction merely of postponing the attack, but when the attack comes it is all the worse. On the other hand, so-called hysterical epilepsy is one of the most readily hypnotizable types of complaint.

VI. HYPNOSIS, ANALYSIS AND TRANSFERENCE

I have had no time to deal with the analytical aspect of the matter, with the question of transference in the Freudian sense. Nothing that I have said is in conflict with whatever views may be held in respect of transference. Obviously all good practice involves a certain amount of analysis, the more analysis the better so far as it is possible in the circumstances, but in many cases the symptoms have to be relieved quickly, the patient has to be helped at the very early stages, and suggestion treatment is justified, even though one may proceed to analysis afterwards, though I admit that the better order is analysis first and then suggestion. Many analysts, indeed, would say that there should be no suggestion at all, that suggestion is an element of weakness, intensifying whatever weakness there be in the subject. Nevertheless, I think suggestion following analysis is sometimes a useful procedure.

The personal relationship between the patient and the psychotherapist (analyst or hypnotist) is itself a psychological problem still awaiting a completely satisfying solution, in spite of the attempts of Freud, McDougall and others to provide a scientific explanation of it (9).

VII. THE PSYCHOLOGY OF LEADERSHIP

From what has been said above, it will be clear that we must distinguish between pathological hypnotism (cf. black magic), involving the arousal of fear and suggestions of inability, and therapeutic hypnotism (white magic), where suggestions of enhanced power and self-confidence are given. A patient in front of a therapeutic hypnotist is not a 'frightened rabbit'. Rather it is a case of the identification of the patient with the hypnotist, whereby the latter is enabled to use his personal influence to inspire confidence and faith on the part of the patient and to give the assertion of health. This has relevance in the wider field of leadership in political affairs. The relation of follower to leader is not necessarily one of mere submissiveness, rather is it one of identification of the follower with the leader. Whatever opinions may be held with regard to the political aims and activities of the Fascist countries,¹ there can

¹ As a philosophic liberal, I find myself strongly critical, from the ethical and political points of view, of the principles and methods of the Fascist countries.

be no doubt that some of the methods adopted in Italy and Germany for attaining efficiency as a nation under individual leadership find a certain support in modern psychology. It is a mistake to suppose that the 'followers' in those countries are slaves, copying their leader from motives of fear or in cringing submission. Rather do their own self-assertive and aggressive tendencies become liberated in the process, and their heightened enthusiasm and confidence in their leader and his resources make what might have been a timid panic-stricken crowd into a powerful army, nation or race. In this respect, although in this respect only, Herr Adolf Hitler is the greatest psychotherapist of a nation, giving Germany national unity, national health and corporate courage, and he will go down in history as such.

Whether he will prove to be a national psychotherapist in the wider and more complete sense, and help to produce peace among the nations and a higher international morality, is a question still awaiting an answer from the march of events. Dictatorship is, politically, a regression, and although sometimes temporarily justified in the face of overwhelming national difficulties, it is never ultimately a satisfactory form of government.

Therapeutic hypnotism, scientifically and skilfully applied, may do the same for the individual. Instead of bringing about self-abasement and psychological automatism, it elicits new powers and makes greater achievement possible by its integrative effect upon the personality.

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AN EXPERIMENT ON INSIGHT

By R. W. PICKFORD

From Glasgow University Psychological Laboratory

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I. INTRODUCTION

HARTMANN⁽¹⁾ has discussed some of the problems of insight in one paper, and later⁽²⁾ has reported an experimental study of insight. He used ten tests in which errors would abound until the 'principle' of solution was discovered, and then cease altogether. Instantaneous solutions were found sometimes, but were often preceded by gross errors. He thinks suddenness has been given too much weight as a criterion of insight. Sudden changes in learning curves may be no more than great accelerations, and although a 'principle' of solution cannot operate until available in its entirety, yet trial and error preceding it may be quite relevant to it.

Mary E. Bulbrook⁽³⁾ got her subjects to solve thirteen problems each involving some form of flash or insight. She points out nine mental processes to which the word 'insight' might be applied. These increase in complexity from perceptive apprehension up to comprehension of a constructive scheme or the rules of an art. She thinks the farther from the first type we go, the looser and the less satisfactory is the use of the term 'insight', and, while it may remain in the description of certain forms of animal behaviour, appeal to it as an explanatory principle will probably be made less and less often.

J. I. Drever⁽⁴⁾ reported an experiment in which the subjects were asked to identify the common feature (in one set a right angle) in a series of simple geometrical figures in which not all members had that feature. The subject responded by pressing one key if he saw the common

feature and another if he did not. When correctly pressed, either key would ring a bell. The subject made statements about the principle, if any, on which he was working at a given time. He was also tested at intervals to find if his reproductions of standard figures might be modified unwittingly by the principle on which he was then working. Although insight did come suddenly, subjects were surprisingly often correct before insight occurred, and as often right as wrong at the moment of insight. The reproductions of standard figures did sometimes show the influence of the principle which later operated in the form of conscious insight. Drever thinks that certain subjects "found, after a period of success, that their mode of response was inadequate. This realization proved sufficient, *without any further clues*,¹ to bring the correct principle into consciousness".

II. METHOD AND APPARATUS

The present experiment was a simplified form of J. I. Drever's experiment. The apparatus consisted of an electric circuit in which either of two similar keys could be connected with a battery and bell by a two-way switch. The keys were before the subject, and so was a card-holder at eye-level. Other parts of the apparatus, the writing materials and the experimenter himself, were screened from the subject, but the experimenter could see the subject, if he wished, by leaning round the edge of the screen.

The experimenter explained to the subject that he would show, one by one, a series of twenty-seven cards, on each of which was a simple geometrical figure; that some of the figures had a common feature, and that the subject was to identify this as they were shown. If the subject saw a figure with what he took to be the common feature he was to press the right-hand key, otherwise the left. He was then allowed to try the keys, and to discover that the experimenter controlled which of them should ring the bell. He was also told that the correct response to any card would always ring the bell. If the subject asked any questions, non-committal answers were given, or, if necessary, he was told that the experimenter regretted his inability to answer the question asked. Most subjects asked what they were to do in response to the first card. They were told to do exactly what they thought best.

The original series of thirty cards was reduced to twenty-seven because three proved unsuitable in preliminary tests. The cards were postcard size and on each there was a straight line figure about 3 in.

¹ Italics in the original.

long drawn in Indian ink. Fourteen of the figures, in the order 1, 4, 5, 7, 9, 11, 13, 14, 16, 19, 21, 22, 24 and 26 contained a right angle. The number of sides varied from three to six. Each card was placed in the card-holder by the experimenter, and removed after the subject had pressed one of the keys. The subject was asked to report briefly why he pressed the right or left key, and to give any further account he wished of what was passing through his mind. The experimenter made a note of all these responses. There were fifteen subjects, and three of them women; three were well acquainted with psychology; one, an art student, knew nothing about it, and the rest knew a small amount. Three of the subjects saw the whole series of cards twice over; two never got insight.

III. STATISTICAL TREATMENT OF RESULTS

The results of the experiment may be analysed numerically, as shown in Table I. The responses to the first card are shown in the second column. In the third and fourth columns are shown the numbers of right and wrong responses before insight. The fifth column gives the numbers of the cards at which insight occurred; the sixth shows the responses to these cards. The seventh and eighth columns show the numbers of right and wrong responses after insight. Pooling the responses of all subjects—probably the best step, although it is open to some objections—we may treat the results as if they were got by tossing pennies. It is then found that the responses before insight might easily have been

Table I

Subject's number	Card 1 response	Before insight		Card 'giving' insight		After insight	
		No. R.	No. W.	Card no.	R. or W.	No. R.	No. W.
1 ¹	Wrong	8	10	19	Wrong	8	0
2 ²	None	8	9	19	Wrong	6	2
3 ²	None	24	20	19 <i>bis</i>	Wrong	7	1
4 ¹	None	3	2	7	Right	20	0
5 ²	None	16	10	—	—	—	—
6 ²	Right	10	11	22	Right	5	0
7 ²	None	6	3	11	Wrong	15	1
8 ²	None	5	7	14	Right	12	1
9 ²	None	15	14	4 <i>bis</i>	Right	23	0
10 ²	Wrong	0	4	5	Right	21	1
11 ²	None	32	21	—	—	—	—
12 ²	Right	4	4	9	Right	17	1
13	Wrong	1	3	5	Right	20	2
14 ²	Wrong	10	10	21	Right	6	0
15 ¹	Wrong	0	1	2	Right	24	1
Totals	{ None	8	—	—	None	—	—
	{ Wrong	5	129	—	Wrong	4	10
	{ Right	2	142	—	Right	9	184

¹ Much psychology.² Little psychology.

got at random (deviation obtained is 0.79 times the standard deviation), and in no way indicate the presence of a tendency favouring the right response. Also the highest single score, 6/9 right by Subject 7, is not statistically significant either as a single result or as one of the group of fifteen. On the other hand the responses after insight indicate a very strong tendency favouring the right response (deviation obtained is 12.5 times the standard deviation).

Drever gives the pooled percentages of correct responses for the pre-insight period of the six subjects who later got insight, and for the eight who did not. These data appear strongly to support his claim that some factor equivalent to insight was at work in the pre-insight periods of subjects later successful, because the percentages seem to indicate a very strong tendency favouring the correct response before insight.

There is no doubt that treating the results of this experiment as though they might have been got by tossing pennies is in many ways altogether unfair. Mr B. Babington Smith has pointed out¹ that many of the subjects had more than one possible common feature in mind during the pre-insight period, and that the chances of using the right idea a given number of times under these conditions are like those of getting one particular face that number of times in tossing a group of three, four or many-sided dice. This modification of the principle would, however, as he pointed out, be very difficult to apply to the present experiment, because it is very hard to say how many ideas a subject actually had in mind, because these ideas would almost certainly be dependent in various ways on each other; and because such ideas tend to be dropped out and taken up in a complicated way; so that one seems to be working with hypothetical dice which have variable numbers of sides. In view of these possibilities, we must say that the statistics calculated for the results of the experiment, and shown in this paper, do not prove that the choices of the subjects in the pre-insight period must be regarded as mathematically equivalent to chance steps, but they do prove that the choices after insight cannot be so regarded. If, however, the pre-insight results had been statistically significant on the hypothesis used here, that two-sided dice were being tossed, then they would have been significant also on the hypothesis that the dice had any greater number of sides.

It must be concluded that the present experiment does not, as far as statistical results go, necessarily reverse Drever's conclusion. It would

¹ At a meeting of the Scottish Branch of the British Psychological Society, 4 December 1937, at which this paper was read.

be most interesting if another experimenter could test Drever's results out in the same or in a different form of the experiment. If the result was supported, carefully collected introspections might indicate how the correct responses before insight were got. In order to handle the numerical data in the present experiment, it has been necessary to assume that insight came to each subject at a definite card in the series. The first card at which he mentioned thinking that the common feature might be or in some way involve a right angle has been taken as the card which 'gave' him insight. It will be seen in the next sections that this step is not fully satisfactory.

As a supplementary experiment five other subjects drawn from a comparable group were asked to do exactly the same as the fifteen already mentioned, but were not told that the ringing of the bell indicated that their responses were correct. None of these subjects got insight, though two of them went through all the cards twice. None of them thought the bell indicated anything whatever about the correctness of their choices; one thought that when it failed to ring the apparatus was out of order. Their results might easily have been got by chance (189 responses: deviation obtained is 0.56 times the standard deviation). This is a remarkable result; their failure to get insight shows how easily the problem can be made insoluble.

IV. QUALITATIVE ANALYSIS OF RESULTS

(a) Steps leading to insight

The apparently random results were almost always got because the subjects were working on wrong principles. On a very few occasions the subject pressed one or other key because he had to do something and did not know which key would be the better. Three responses to the first card were deliberate experiments, to find out whether it had the common feature or not, and an occasional response was made just to show the experimenter that the subject had seen the card. If the report of Subject 1 is taken, he is found to have used the following principles tentatively in response to the first nineteen cards: "figure not completed"; "completion of triangle"; "general tapering and acute angle to right-hand corner"; "something with angle pointing down"; "re-entrant angle"; "four sides". Some subjects did not have as many ideas as this, but it is a typical series in other ways. The pre-insight period is occupied, not with random trial and error, but with very hard directed thinking, in which the subject imagines and then tests out one hypothesis after another, in the hope of finding the solution.

The final steps towards insight, however, are often taken in an interesting way. Subject 1 approached card 19 with the comment, after successfully pressing the no-key for card 18, "Want another four to complete theory that four-sided figure is correct." At card 19 he was wrong in pressing the no-key, and said, "Evidently not. Idea it might be right angles." At card 20 he correctly pressed the no-key, and said it was "obviously" not a member of the series he was looking for, though he added, "Don't know why I said 'obviously'." At card 21 he correctly pressed the yes-key and said, "That's a right angle." At card 22 he said, "To see if right angle must always come in left-hand corner, I'm going to try that!" He then rightly pressed the yes-key, and added, "Right angle can come anywhere." After that he was always right, and at card 24 he was sure that the right angle was the common feature. For statistical purposes it was necessary to decide at what card insight came to Subject 1. Card 19 'gave' him insight, because this was the first place he mentioned thinking the common feature might be a right angle. However, he did not expressly use this principle until card 21, though it seems to have helped him to an unwittingly correct response to card 20, and the principle was not separated from ideas about the position of the right angle until card 22. It was not definitely accepted until card 24. We must conclude that there were several steps in the coming of insight, and that these were not all consciously taken. In the responses to these cards, 18-24, Subject 1 was wrong once, at the very card which 'gave' him insight. Results essentially similar to these were got from Subjects 2, 4, 6, 7, 8, 11 and 12, which indicates that the present experiment may support J. I. Drever's conclusions more fully than the statistical treatment of the results would suggest.

Of the seven subjects not included in this list, two never got insight, and of the remaining five subjects 3 and 14 seem to have gained insight quite suddenly without anticipating it. Subjects 10, 13 and 15 reached the correct solution by processes essentially similar to that described for Subject 1, but much more rapid. They were fortunate in an early choice of the right angle as a likely hypothesis (which might itself be due to a form of insight), and in avoiding the continued use of hypotheses amply disproved.

It is quite legitimate to speak of the first card at which the subject is thinking in terms of the principle which later proves correct as the card which 'gives' him insight. At other cards, however, he is first thinking in terms of principles which are later found to be incorrect. Subjectively, and at the time of the experience, the incorrect might seem

as much like true insight as the correct principle. The idea or principle may flash into mind after unconscious preparation, or it may come into consciousness by degrees, but it is still to be tested out before the subject can be said to have insight fully developed. The full development of insight therefore seems for the most part to take time, though many degrees of rapidity may be observed.

(b) General nature and conditions of response

Even subjects who were irritated at the apparent triviality of the experiment found it decidedly interesting, and most subjects were quickly on their mettle and out to solve the problem. It would be absurd to treat the coming of insight as purely cognitive. Subject 4 remarked, at card 3, "Oh! Damn! I forget all the others"; at card 7, "Curse it! What about the right angle?"; at card 8, "Ach! Oh! Dear! Doesn't belong"; at card 10, "I'm putting all my money on the right angle. It seems to lose a good deal of its interest when you think you've got the solution. There is a sort of anxiety before you discover it." After that she was always right. Subject 5, who never got insight, remarked, after the experiment was over, "When I saw the geometrical shapes I had a sinking feeling. I thought there would be something very deep in this and I wouldn't get it. You know the sort of feeling you get." These are extreme examples, but it is clear that only a most unusual subject would react in a purely cognitive way. Even Subject 15, whose responses were more highly reasoned and calculating than those of any other subject, showed great interest in what he was doing and appropriate satisfaction or the reverse when he succeeded or failed. In general it may be said that there were any number of intermediate types between the subject who showed strong emotional responses freely and the subject who appeared to be taking purely rational steps. There is also the subject whose emotional reactions make the experiment an extremely unpleasant task, and who, accordingly, often fails to grapple effectively with the problem set.

The peculiar form of the experiment made it an exercise in applied logic, or, perhaps, in scientific method. Other experiments on insight, some of which the writer hopes to do, might give a less exaggerated emphasis on logical method, and it would not be fair to conclude from this experiment that all insight is gained by such logical steps as are shown here.

The methods adopted by different subjects are of great interest. Subject 15, who was both a good mathematician and experienced in

psychological experiments, adopted almost the ideal attitude and method. He realized that he should experiment with one of the keys at the exposure of the first card. He pressed the no-key, "to see what would happen", and was wrong. He added, "I notice one right angle, all angles different, no sides the same, and four-sided figure". At card 2 he pressed the no-key, "on the ground that there was no right angle". "It seems to be worth trying the right angle as the common factor." At card 3 he pressed the yes-key wrongly, thinking "that might have been a right angle in right corner". After that he was always right, but not until card 12 did he find himself "not considering other possibilities seriously and not looking out for other points". In spite of his extraordinary efficiency, his insight remained subject to his own estimate of his success and correctness for about ten cards. This occurred with at least one other very efficient subject. Scepticism is not always incompatible with action.

A somewhat less efficient method is used by subjects whose logic is unimpeachable, but who do not make the necessary observations, and do not experiment with the first card. Subject 8, who is a teacher of logic, did not think of doing anything with the first card. He assumed that, since he could not tell whether it had the common feature, he could not be expected to press either key. At card 11, after many failures, he was wrong in pressing the no-key. "Well!" he said, "Now what was that based on?" "That was based on angles and that there was not a right angle in the original figure, and that the other angles were much too acute." At card 14, however, he correctly pressed the yes-key, and said, "Based on a revision that there was not a right angle in the first figure, and the assumption that the common feature was a right angle."

An even weaker method involves failures both of logic and of observation. This is illustrated by Subject 11, who saw all the figures twice without getting insight. At card 1 she remarked, "I can't do anything there." At card 2 she was wrong in pressing the yes-key, and exclaimed, "Oh! I thought it had. Thought it was four sides." At card 3 she made exactly the same mistake, still thinking the common feature was four sides. At card 4, still working on the same hypothesis, she was right because the card shows a four-sided figure containing a right angle. After this very unfortunate start she became interested in the number of sides, the direction in which the figure was pointing, the rhythm, harmony of structure and other features. She explained to the experimenter afterwards, when discussing the figures, that she was unable to distinguish the right angles(!). Many of her introspections were exceedingly

interesting, but lack of space forbids their quotation. Subject 5 had the same kind of difficulties; both dislike mathematics strongly, and regard problems about geometrical figures as a very difficult part of that science. Subject 5 never got insight, and at card 26 was working on "Same principle—sort of quadrilateral you'd call that, wouldn't you?" This principle had been proved wrong at card 2, and in the interval she had tried a variety of hypotheses. The obstinacy shown by her return to the original hypothesis, in spite of its failure, is also shown in other ways. At card 9 she asked, "Can you change your idea?" She was told that she could. At card 18, she remarked apologetically, "I'll have to change the principle, I can see." Her own resistance against changing her mind seemed to be projected on to the experimenter, and his permission was needed, or his fancied objection had to be overcome, before she could think constructively.

Another observation concerns Subject 3, who saw all the cards twice over, and did not gain insight into the principle of the right angle until card 19 was presented for the second time. He looked for "similarity" between each card and the card before it, and was also interested in the re-entrant angles. After the experiment he was asked if he could give any explanation of the long delay of insight. He replied that for much of the time he had not been noticing the bell; he said he knew quite well that the ringing of the bell would show him that he was right, but he had not connected it up with the ideas in his mind. Three weeks later this subject was required to give up work temporarily because he was thought by a competent psychotherapist to be approaching a serious schizophrenic breakdown. The breakdown was avoided and at the time of writing this paper he had recovered.

These emotional and schizophrenic complications suggest that the psychopathology of insight may be less remote from the psychology of normal insight than might be supposed. Insight in abnormal subjects cannot be treated simply as something which neurotics have and which is lacking in psychotics. An interesting discussion of the psychopathology of insight has been published by Lewis(5), but it is not possible to deal with the subject further here. Experimental psychologists might, however, with advantage, take more account than they do of the relations between their findings and those of psychopathology.

A very interesting point is the influence of cards 5 and 19, which showed obviously right-angled triangles. Subjects 1, 2, 3, 10 and 13 gained insight at one or other of these cards. Subjects 5, 6, 12 and 14 seem to have been much influenced by seeing one or other right-angled

triangle, or to have found themselves thinking about right angles shortly after seeing these triangles. There is no doubt that, for a number of subjects, one right-angled triangle or the other came after a number of failures, and acted as a cue which set them thinking of right angles. Sometimes this brought the sudden achievement of insight already prepared for; sometimes it was just a step towards insight to be achieved later. This suggests the possibility of an experiment in which the positions of such cues might be varied at the discretion of the experimenter.

The responses made by the five subjects who did the experiment in its modified form, in which they were not told that the ringing of the bell indicated a correct response, are most interesting. One of them worked mainly on the basis of resemblance to the first figure. At card 4, for instance, he correctly pressed the yes-key on "Basis of angles and that it looked like the reverse of first one." He said at the end, "Thought when the bell didn't ring there was something wrong with the connections." Another of these five retained three principles to the end of the double series. "That's our friend the thirty-sixty degree right-angled triangle" he remarked at card 19. Another principle was, "Five sides—two sides parallel"; the third was, "Four-sided figure with right angle." The most remarkable subject decided at figure 2 that the common feature was the possession of four sides. He never gave the problem a thought again, and when asked at the end of the experiment, "How did you know when you were right?", he said, "I counted the sides." He thought the bell was rung to distract him.

I should not think of attempting a final definition of insight here. Very tentatively, however, it might be said that insight is a conscious grasp by the subject of relations between his own ideas, impressions, feelings and impulses when he is in a given situation; between parts of the situation or environment itself which are significant for him; and also of the nature of this significance. Such insight might exist in various degrees of completeness.

(c) Social aspects of the experiment

It would be absurd to suppose that an experiment of this kind does not involve any social influences whatever. Even if the experimenter could have no contact with the subject during the work, or could remain unknown to the subject or nameless, he would still be a representative of science—a major social institution. In actual practice he is present, though screened from the subject, gives instructions, makes notes and

has to avoid giving conscious or unconscious indications of satisfaction or the reverse in the subject's responses. It would be absurd to suppose that the subject, even if very experienced at such work, can be quite unaffected by the experimenter's presence, by his own attitudes, often unconscious, to the experimenter, or that he does not strive even unwittingly to find out the solution by methods other than those formally prescribed. A naïve subject openly asks crucial questions; a more experienced subject unintentionally listens to the experimenter's movements and tries to see him; an expert subject is more able to overcome these very natural tendencies, but still cannot be altogether unaffected by the social situation. These social aspects do not necessarily make an experiment abortive, but it is very desirable that both subjects and experimenters should be perfectly awake to them. Almost nothing, moreover, is known scientifically about how they affect experimental results, and there is room for research here.

V. SUMMARY AND CONCLUSIONS

A short experiment was done in which fifteen subjects were asked to find the common feature, a right angle, in fourteen members of a series of twenty-seven simple straight line figures. The subject pressed the left-hand key (no) or the right-hand key (yes) before him, and if correct a bell rang. The experimenter made notes of these responses and of the reasons given by the subject for his actions. In the main experiment the subject knew that the bell would ring when he was correct; in a supplementary experiment, with five new subjects, he did not.

Simple statistics which were calculated for the results reverse the conclusion of a similar experiment by J. I. Drever, in which a strong tendency to press the correct key was found before the successful subjects had conscious insight. These calculations are inadequate, however, and if an adequate treatment could be made the results might support that conclusion. Study of the individual responses, and of the reasons given for them, certainly supports Drever's conclusion in so far as insight seems to develop by steps, some of which are not consciously taken. Large differences in the suddenness of insight were found. Examples of the methods used by different subjects are discussed, and the importance of being awake to the social aspects of the experiment is mentioned. It seems that the pre-insight period is occupied, not with random trial and error, but with hard directed thinking in which feelings play a part.

My thanks are due to Mr J. I. Drever, Mr T. M. Martin and Prof. F. C. Bartlett for their help, and to my students and friends who acted as subjects.

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THE THRESHOLD OF UNCONSCIOUSNESS

By C. R. MARSHALL

- I. *Introduction* (pp. 424-425).
- II. *Reaction times* (pp. 425-427).
- III. *Changes of mentality* (pp. 427-429).

I. INTRODUCTION

In the previous paper¹ the effect on 'remembering' of severe intoxicated states maintained at a uniform level by the inhalation of anaesthetizing gases of definite concentration is described. In the present paper certain reactions and behaviour which occur on the borderline of consciousness are discussed.

Table I shows the general course of the intoxication, as indicated by a few obvious symptoms, produced by the smallest concentration of the substances chiefly used which induced unconsciousness.

Table I

General effects	70% nitrous oxide	60% ethylene	35% acetylene
Commencing intoxication	35 sec.	30 sec.	25 sec.
Talkativeness	1½-2 min.	1½-2 min.	1½-2 min.
Noisiness	3-3½ min.	3½ min.	2½-3 min.
Inco-ordination	4-5 min.	5 min.	4-5 min.
Unconsciousness	4½-5½ min.	5½ min.	5-5½ min.

Qualitative differences in the intoxications were slight and of no moment from the view-point of the research; ethylene and acetylene intoxications appeared to produce less excitement during the period just preceding unconsciousness. The mixtures for inhalation were prepared, as stated in the previous paper, in large gas bags, the largest having a capacity of over a thousand litres.

In intoxication there is a slowness of cerebration and a diminution in skill for unaccustomed movements roughly proportional to the severity of the intoxication. With the onset of severe intoxication there is also a qualitative change of character which will be noticed later. Disinclination for work may be said to characterize all grades of intoxication.

¹ "The influence of moderate and severe intoxication on remembering." This *Journal* (1937), xxviii, 18.

This disinclination became almost overpowering in intoxication of a degree induced by 50% nitrous oxide. Without any stimulus to work one tends to pass into a state of complete mental inactivity. In this degree of intoxication inco-ordination of movements becomes noteworthy and with it begins a loss of all sense of self. My instructions to my two assistants were to maintain my attention to the work in hand as far as possible. Whatever merit these experiments possess is due to the fact that my instructions were faithfully carried out, and that it was my earnest endeavour to accomplish a maximum amount of work. Since I was familiar with the effects of these gases there was an absence of anxiety or fear and also of any local action or reflex effects, while the method of administration ensured a constant environment to the cerebral cells when this was desired. Actually most studies of the borderline were taken at minimum concentrations of gas inducing unconsciousness.

II. REACTION TIMES

Reaction times were the only measurable data investigated in this interval between inco-ordination and unconsciousness. It was found for less severe states of intoxication that almost invariably the reaction times, when taken from the commencement of the inhalation, increased in length until a little after a uniform intoxication was established, when they gradually shortened slightly until a practically uniform average level of performance was reached. Reaction times with choice were more influenced than simple reaction times. With increasing intoxication the occasional large variations from the average became larger and more frequent until the stage under consideration when, owing to inco-ordination, the responses tended to be violent. Considerable persuasion and even intimidation were then often required to obtain them. The experiments with choice reaction times only need be given. The choice was between a red and a yellow or green flash. The stimuli were at rapid (to maintain attention) but irregular intervals. A normal record (in seconds) is:

0.26	0.26	0.28	0.26	0.30	0.32	0.24	0.26	0.24	0.26
0.36	0.24	0.26	0.24	0.26	0.26	0.26	0.26	0.26	0.26

The terminal responses under 60% ethylene (0=no response; x=wrong response) were as in Table II.

The inhalation of 35% acetylene induces unconsciousness; 33% acetylene will generally do so if tolerance to it has not been established. In one experiment with this concentration consciousness was allowed to

Table II

Time after commencement of inhalation of 60% ethylene		Reaction times in seconds							
m. s.	m. s.								
3	30—4 30	0.44	0.46	0.64	0.38	0.42	0.44	0.84	0.44
		0.58	0.42	0.86	0	1.06			
4	30—5 0	0	1.04	0.94	0.70	0.82	0.90	0.94	0.48
5	0—5 10	0	0	0	x	x	0	0	0
5	20	Unconscious							

be regained on five occasions by opening the air valve for 1 min. Choice reaction times were recorded in a similar manner on each occasion. Unconsciousness was induced first in 6 min.; after opening the air valve for 1 min., unconsciousness followed readministration in $3\frac{1}{2}$ min. The results in Table III were obtained before the second unconsciousness.

Table III

Time after commencement of inhalation of 33% acetylene		Reaction times in seconds							
m. s.	m. s.								
9	30—10 6	0.90	0.76	x	0.80	0.84	x	0.90	1.0
		0.86	0.76	1.0	No further replies				
10	30	Unconscious							

The same inhalation given 2 days later did not produce distinct unconsciousness. The last reaction times taken were as in Table IV.

Table IV

Time after commencement of inhalation of 33% acetylene		Reaction times in seconds							
m. s.	m. s.								
5	0—6 20	0.40	0.40	0.76	0.46	0.84	0.50	0.52	x
		1.20	1.14	1.42	0.66	x	x	x	0
		x	0	x	x	x	x	0	0

Vision was apparently impaired at this stage. The terminal mistakes suggest that only the light (not noted but probably green) to which response was not to be made was seen. Later a sixpence was shown but was only recognized as a coin; a knife was called a pen. No further responses were obtained to any stimulus. The 9 min. record says "bordering on unconsciousness".

The next morning the same concentration (33% acetylene) was

inhaled for 30 min. without producing unconsciousness. The choice reaction times taken showed the usual slight recovery after the worst depression (see Table V). The normal taken just before the inhalation was 0.26 ± 0.04 sec.

Table V

Time after commencement of inhalation of 33 % acetylene		Reaction times in seconds										
m. s.	m. s.	0.30	0.28	0.30	0.36	0.28	0.32	0.30	0.40	0.34	0.34	0.40
1	0—2 0	0.38	0.36	0.44	1.14	0.30	0.48	0.42	0.40	0.42		
2	0—3 0	0.60	1.20	0.84	0.68	0.66	0.68	1.50				
7	0—8 0	1.24	0.50	0.62	0.64	0.52	0.40	0.80	0.44	0.72	0.52	
8	0—9 0	1.50	0.80	0.70	0.40	0.52	0.64	0.38	0.42			
10	30—11 30	0.76	0.56	0.88	0.52	0.98	0.50	0.44	0.52			
11	30—12 30											

A few items from the protocol show the mentality in this intoxication:

13 min. Difficulty in seeing figures written on paper. Performed a very simple addition but was unable to multiply.

15 min. Recognized pen, knife, book, indiarubber; could not name leather strap; had difficulty with matchbox.

16½ min. Seemed to talk of "playing the game". Wrote it (Fig. 1).

29 min. Asked to write his name. Unable to complete it (Fig. 2).

31 and 32 min. A second and third attempt to write name (Fig. 3).

The inhalation then terminated.

In an inhalation of nitrogen to unconsciousness, which occurred in 1 min., the following responses, commencing 30 sec. after the beginning of the inhalation, were obtained: 0.32, 0.32, 0.32, 0.38, 0.34, 0.42, 0.36, 0.48, 0.30, 0.44, 0.42, 1.38, 0.46, 0.40 sec., x, x, 0, 0, 0, 0. The wrong replies were to green light.

The greatest effect on reaction times was obtained in the first experiment when simple reaction times were taken during an inhalation of 70% nitrous oxide to unconsciousness. The following responses (in seconds) were given in the 3–4 min. period following the administration:

0.42	0.40	0.36	0.40	0.42	0	0.36	0.30	0	0.64	0
0.82	0.38	0.36	0.44	0.42	0.42	0.36	0.36	0.78	0.90	0.38
0.44	0	1.6	0	2.0	0	3.1	0	0	0	

Unconscious at 4 min. 30 sec.

III. CHANGES OF MENTALITY

It has been stated that a qualitative change of character occurred with the advent of severe intoxication. It had a character of omniscience and commenced with an exaggerated estimate of one's own abilities.

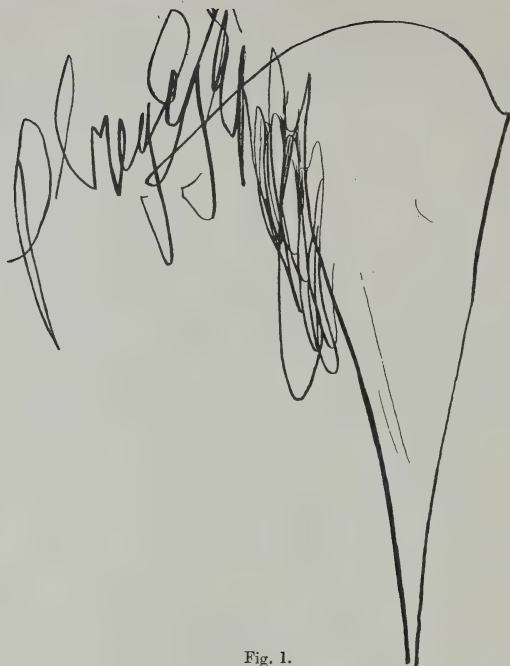


Fig. 1.

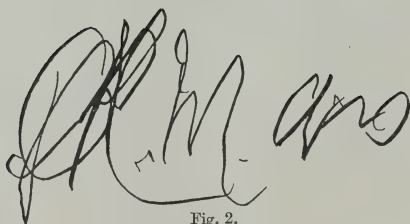


Fig. 2.

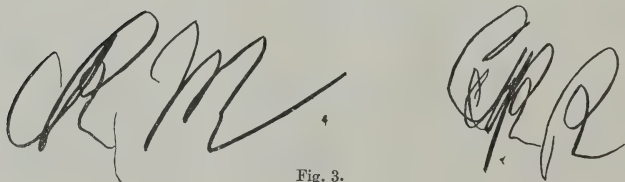


Fig. 3.

During an intoxication induced by 20% acetylene I said "I think I am unusually smart in giving the responses". These, which were simple reaction times, were actually increased 23%. The 'omniscience' appeared during intoxications produced by 50% nitrous oxide, 40% ethylene, 25% acetylene, 10% methyl ether, and 2.5% ethyl chloride, all of which are similar in degree. One example will suffice: $7\frac{1}{2}$ min. after commencing the inhalation of 25% acetylene I wrote "Intellectually I feel as keen as ever", and 8 min. later "I feel capable of conducting the Government". It was found that I was unable to do a simple multiplication by 9. On another occasion I apparently thought that if I were Foreign Secretary I could set the international muddle aright. On one of the protocols my assistant has kindly written "This egoism is in curious contrast with his normal state".

Rarely a sense of inferiority was noted. I am credited on one occasion with having said to my junior assistant "To me you seem extraordinarily clever"; and on another occasion I wrote "I can do nothing with alacrity". Once on the verge of the threshold I wrote "Time is beginning to have no meaning for me". As far as my memory serves the effect experienced after the balance of a 60% nitrous oxide inhalation had been attained was one almost of non-existence until some task was requested and attempted. Perhaps it would be more correct to say that alterations of slightly varying states of more or less just perceptible existence occurred, since one was somewhat more lucid at one period than at another. On recovering from the unconsciousness of 70% nitrous oxide to the consciousness of 50% nitrous oxide in a continuous experiment I had the curious experience of an awakening from death to life. This I have never felt during the many recoveries from unconsciousness by breathing air. Nor in these recoveries from unconsciousness induced by nitrous oxide did I ever experience those brilliant conceptions ("The Flash of Genius") which, however, cannot be recalled; but late in the research I became imbued with the thought, especially in the early morning after awakening from sleep, which became almost a fixed idea, that the experiments I had been conducting were of great educational value.

With the approach of unconsciousness loss of sight occurred before loss of hearing; understandable replies could be obtained after there was inability to write them. The last responses appear to have been wide movements of the arm in an attempt to slap the knee.

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SOME CHANGES IN SOCIAL LIFE IN A COMMUNITY WITH A FALLING INTELLIGENCE QUOTIENT

BY RAYMOND B. CATTELL

- I. *Origin of the problem* (pp. 430-431).
- II. *Limits of the discussion* (pp. 431-432).
- III. *Methods of solution* (pp. 432-433).
- IV. *Nature of changes deduced* (pp. 433-449).
 - (a) *In education* (pp. 433-435).
 - (b) *In economic life* (pp. 435-438).
 - (c) *In moral standards* (pp. 439-442).
 - (d) *In political and social life* (pp. 443-444).
 - (e) *In general culture* (pp. 444-449).
- V. *Summary* (pp. 449-450).
- References* (p. 450).

I. ORIGIN OF THE PROBLEM

FROM a recently completed survey (5, 7) of reproduction rates obtaining at various levels of intelligence in the population of Great Britain, I have drawn the conclusion that, at the present moment, the average level of native mental capacity is falling at the rate of approximately one point of I.Q. per decade.

This conclusion depends upon two premises: (1) that throughout the whole intelligence range the average size of family for each intelligence class is inversely related to the intelligence level, and (2) that, although the gene complex determining individual intelligence is far from being understood, the facts of psychological measurement indicate that, even for quite small groups, intelligence of children may be predicted from intelligence of parents, as if mental capacity were a product of heredity.

Though the observation that mental capacity is largely inborn has long been accepted by the majority of psychologists, it has not always been so favourably received by workers in sister sciences. The truth of the observation need not be discussed afresh here, since the evidence up to date has been comprehensively summarized elsewhere⁽⁶⁾, but it is relevant to point out that even the most generous allowance for environment which those wishing to stress environment have dared to claim, would fail to account for more than a small fraction of the variability of intelligence quotient existing in our population.

Calculations of the differential birth-rate based on surveys in rural or urban areas remote in space or culture from those investigated in this first enquiry, or separated by an interval of time sufficient to permit of changes in birth-control practice, may cause us to modify the above figure for rate of decline. Consequently the present study will not depend upon the assumption that the figures are accurate. Indeed it will not depend for its *raison d'être* on the assumption that any decline at all has been proved, but will take its justification from the fact that the relation of community mental capacity to social and cultural habits has been grievously neglected in both discussion and investigation, in spite of its great theoretical and practical interest.

Should the above survey be confirmed in its findings, this hypothetical treatment of the relation of culture to the biological quality of the units constituting the group would become relevant to practical politics, for the methods of combating the decline must depend upon a sound analysis of cause and effect among the conditions accompanying it.

II. LIMITS OF THE DISCUSSION

Social consequences might conceivably follow either from changes in the absolute level of intelligence, the distribution form remaining constant, or from changes in distribution, e.g. standard deviation, the average level remaining unchanged.

Both kinds of change appear to be taking place, for a bulge in the distribution curve at the 70-90 i.q. level would increase standard deviation if, as seems likely, the upper intelligence levels are passing out of the phase of greatest family restriction; but in a first attack on the problem it seems best to limit the discussion largely to the downward shift of the distribution curve as a whole.

To argue that certain social changes must of scientific necessity follow the posited changes in biological character is not to expect that they will do so as a matter of historical fact. For intelligence is only one of many independent factors, and the cultural influences among the latter, e.g. the influences of individual leaders, are more rapid and unpredictable in action though, of course, their action is itself modified by the factor we have to consider here. Secondly, the primary social and economic consequences of the intelligence change will interact among themselves, masking, transmuting and producing consequences of a second order. These consequences are too numerous and complex to be discussed in a first study. Moreover it would be premature to attack them until the first set of direct consequences has been approved

by argument and research. Except in rare instances the discussion will therefore confine itself to the first and direct influences of a falling intelligence level.

III. METHODS OF SOLUTION

To look for the effects entirely, or even mainly, in the cognitive field—in educational, academic and technical industrial efficiency—merely because the variable we are dealing with is a cognitive one, would produce an altogether false solution of the problem.

Clinical experience has convinced the present writer that the researches of, for example, Burt (1), Healy (10) and Terman (16), showing mental capacity to be one of the more important factors determining the individual's character development, tend to understate, by simplifying, the extent to which character education is limited and modified by the individual's mental capacity. If this is true of the individual, it must apply even more powerfully in society, which to a far greater extent creates its own environment. For, apart from the heritage of skills and intellectual furniture handed down from previous cultures and now almost constant the world over, the mental capacities of the members of a group (and whatever other psychological qualities, if any, are fixed by inheritance) are themselves the environment shaping the formation of character and limiting the patterns of emotional adjustment in living individuals and in the coming generation. It is true that this heritage, which may seem disposed of over-lightly in the above sentence, includes the use made of the (constant) physical world and also the economic system, its level of energy supply and its structure. But these, in a community which has long had various alternatives open to its choice, must in the end be selected largely according to the community intelligence level.

The fields in which these effects will be felt and their particular character must, of course, in the end be decided by experimental and statistical methods, like any other problem of social psychology. Suitable experimental situations may not easily be found, for groups of differing intelligence average generally differ also in racial type and cultural history, whilst to pick out groups within the same race and nation does not provide all the conditions required, for such groups would rarely have had any extensive control over their own legal, economic and social customs. Studies of a sufficient number of groups, even if differing racially and in social heritage, should, however, permit some general conclusions to be drawn, just as the gathering of data on a sufficient number of

individuals will enable one to eliminate all individual differences other than that to be studied.

There is no reason why valid answers to some issues should not emerge from experimental study on a relatively small scale of groups of children, equated in age and other respects but differing by a known amount in average I.Q. or standard deviation within the group. Observations could be made on the qualitative differences in their manners of organization, rules, customs and moral standards. Quantitative results could be obtained by comparing their scores in competitive games and community projects; whilst the technique for measuring attitudes, interests and allied questions of energy disposal, would almost certainly yield interesting general laws.

Doubtless the findings would be far more surprising than any to be deduced from armchair analysis. The present writer once had the opportunity of seeing a team of mentally defective youths play at football a team of normals of the same age and weight. The former won rather easily, this result being seemingly due to the fact that, thwarted in mental expression, they had given more time to acquiring physical skills, and also to the more ready renunciation of individual success to which these youths had become accustomed earlier and which gave great solidarity to their team work.

This article, presenting a first approach to the problem, must unfortunately depend largely on deductive reasoning, employing our knowledge of the nature of mental capacity and an analysis of social situations. But there are already, here and there, valuable empirical studies connecting intelligence and behaviour in special fields of social life, and upon these we can draw for the construction of foundations. These fields are the applied sciences of industrial psychology, child guidance, and educational psychology, plus a few specific studies in social psychology. Since the consequences in the educational field are relatively certain and clear cut, and since effects elsewhere to some extent ensue naturally from them, it would be best to consider education first.

IV. NATURE OF CHANGES DEDUCED

(a) *In education*

Probably the most significant improvement of our educational system during the past decade has been the progress of the precise classification of children according to real ability, extending from the special schools, through the C, B and A classes of the elementary schools, to the A classes

of selective secondary schools. In these distinct streams the children proceed at very different rates for, as Burt has shown, the variability in mental age tends to result, at least in some of the main school subjects, in an even greater variability of attainment age.

But of greater significance to society is the fact that in the 'child-centred' school of to-day the curricula and methods are adapted, not only in levels but in quality and range, to the capacities of the children; for interest is a function of capacity and teaching without interest is futile unless tyranny is invoked. This means that any alteration in the numbers of children born at the various intelligence levels will lead inevitably to changes in the proportions of young citizens having the various kinds of training and curricula. To take an instance from one end of the scale: the 24 % increase predicted in the numbers of feeble-minded will result in a similar increase in the school leavers trained only in crude handwork, barely able to read and unable to carry out elementary calculations. The modern child-centred school only serves to bring out more clearly the truth inherent in all educational systems: that the standards and types of education, when the community is giving the best it can to all children, are at the mercy of birth-rates.

This general change, in which those fitted for advanced and abstract studies dwindle whilst those profiting only by simple, concrete education increase, needs next to be considered quantitatively with regard to the maintenance of scholastic standards in all types of school, with their existing curricula and goals. The fall of intelligence with which we have to deal amounts to six months of mental age in a generation. Such a fall might be offset by greater time given to the main subjects of education, either by economising on the unfortunate 'frills' or by increasing the length of school life.

Although the results of education can closely simulate native wit, the product of substitution will not be quite the same.¹ Proficiency in such subjects as arithmetic and English (notably style and vocabulary) correlates much more closely with 'g' than does proficiency in simple manual and mental skills. Consequently in the former a point is soon reached in the decline of intelligence beyond which insufficient intelligence is compensated for by a quite exorbitant expenditure of time, or not at all. Teachers who complain that an extra year or two of schooling will

¹ As every academic teacher knows, education beyond a student's actual capacity can have a sterilizing effect on the mind and a stultifying effect on personality; but these psychological complications cannot be followed up here. Whilst this article was being written a landlady, describing a visitor to the writer, said, in good faith, "He was an educated man, but not stupid..."

be wasted on a certain proportion of their charges are naturally referring to abstract and conventionally 'cultural' subjects, not to manual skills.

The ultimate consequences in education have not been considered until we look at economic effects. Since the feeble-minded child costs £36 per annum (the residential defective costs far more) against £12 or £13 for the average child, and since at every level the less intelligent pupil costs more (whilst never reaching the same finished level), existing scholastic standards cannot be maintained without deflecting more state expenditure into education. And this sets up a chain of more remote effects through the economic drain upon the other social services.

(b) *In economic life*

Recent studies^(4,7) in this country and several in America have provided us with knowledge of the distribution of intelligence in various occupations. There is a definite and significant grading of average i.q. according to the complexity of the occupations or to the level of rewards to be expected from the occupation.¹ A good deal of overlap in intelligence exists,² the lowest quartile of the medical doctors, for example, being at the same level as the highest quartile for precision fitters or shop assistants.

At first sight we might suppose that a fall in the average intelligence quotients, with the distribution form remaining constant, would result in a shortage of men with suitable capacity for the more skilled or 'professional' occupations, and an excess of those able to absorb only that minimum of lore necessary for unskilled labour.

This assumes that in most occupations men have already reached the limits of their educability. Such a saturation point in education has perhaps been reached in the professions, where men fail to be better teachers or doctors not through any lack of training but through their own limitations, but it is scarcely true of, for example, the ranks of unskilled labour, in which, as our scatter diagrams show⁽⁴⁾, there is still undoubtedly a fair proportion of men of high capacity not using that capacity.

Because of this last-mentioned fact, the dwindling of the actual supply of natural capacity could easily be met for a generation or more

¹ Substantially the same order is found for the children of fathers in these occupations, but with more overlap between groups, probably owing to the regressive effect of the mothers, a result of incompletely assortative mating.

² It would be a mistake to assume that the scatter of 'total effectiveness' is as great as the scatter of 'g'. Among those successful in teaching, for example, it seems that lower intelligence is systematically associated with better temperamental endowment^(2, 4).

by a better utilization of available ability. Gray & Moskinsky⁽⁹⁾,¹ among others, have shown that in our school population only about half of those with secondary school ability actually go to secondary schools.

On the other hand a more thorough combing of the school population, a more complete extraction of good ability from the unskilled occupations, may lead to costly inefficiency where we least expect to find it. For it is a mistake to assume that the occupation requiring the longest training is necessarily that requiring the highest general ability. A good foreman or charge hand needs a high level of general intelligence for the complex human and material problems which not infrequently crop up. And even in mechanized factory work, as G. H. Miles⁽¹³⁾ has recently pointed out, "countless . . . operations have arisen which demand little bodily activity and much specialized mental effort and concentration". It may be more important to avoid incompetent surgeons than incompetent railway foremen, and this combing of the population for good brains is indicated also by social justice; but it is not a solution of the problem of declining intelligence, even temporarily.

Though it is obviously untrue to say that in all occupations men are called upon to behave in ways characteristic of their intelligence at its upper limits, they are, in a great number of acts and decisions, working close to this upper limit. Any fall of intelligence might be expected to involve industry in a disproportionate increase in training to compensate for deficiencies or to avoid difficulties which would not have arisen with the more self-sufficient worker.

When such education is not given, or where it is useless to seek in education compensation for the lack of native wit, the result of a heaping up of individuals at a certain low intelligence level will be unemployment at that level. In all branches of industry, commerce and administration, a certain ratio of the number of highly trained to less trained has been established by the nature of the work to be done. The employment of a certain number of men of lesser ability requires the catalysing and directive influence of men more gifted. A falling off in the numbers of the latter means unemployment for the former.

Looking at the matter more broadly, we may say that any given culture establishes a certain distribution curve of demand for intelligence at various levels, and that harmony must exist between this curve and the curve of supply given by the birth-rates at various intelligences.

¹ These workers obtained 42.9% as going to secondary schools, but their age group was from 9.0 to 12.6 years whereas 11+ is the normal age for transferring to a secondary school.

Originally these curves must have been identical, for a society could not invent or borrow a culture more complex than it could understand. But dislocations might take place through alterations in either of the curves. A widespread earthquake, for instance, or a war, by creating immense demands for relatively unskilled labour, would be inappropriate to the present curve of supply (Fig. 1), but any bulge in the lower part of the

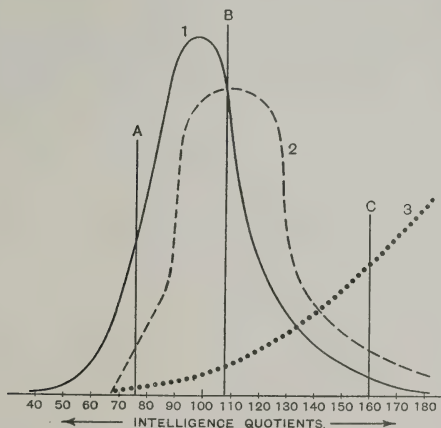


Fig. 1.¹ A. Supply greatly in excess of demand at 75 i.q.—negligible wage. B. Supply and demand equal—normal wage. C. Demand greatly in excess of supply—disproportionate wage. (1) ——— Distribution of actual i.q.'s. (2) - - - - Distribution of demand for i.q.'s. (3) Rate of rewards arising from relation of (1) and (2).

intelligence distribution curve such as is caused by the present change in the curve of birth-rates would adjust to such a curve of demand. In stable, progressive conditions of civilization, on the other hand, such a change in the curve of supply must result in unemployment being greatest at the lower levels of intelligence.

In confirmation of this we may consider Fig. 2, showing the intelligence levels of children of unemployed in the two areas surveyed in the original research. It may be objected that in any period of unemployment it is only natural that the more intelligent will in competition secure jobs; but it is yet to be proven that these are too intelligent for the jobs they take.

¹ I am indebted to the *Eugenics Review* for permission to reproduce the diagrams shown in Figs. 1 and 2.

Students of population have long seen the errors of the naïve view that unemployment is due to overpopulation; next they have passed to studies of the economics of production and distribution; but it is possible that they will find at the root of the economic problem a psychological problem arising from the social effects of too great a range of innate individual differences in mental effectiveness.

Any attempt to discover the more direct effects upon wealth of changing intelligence levels must rest upon generalities. Recent work has shown a correlation of 0.9 to exist between the earning power of occupations and their mean intelligence demand (7). It has been argued

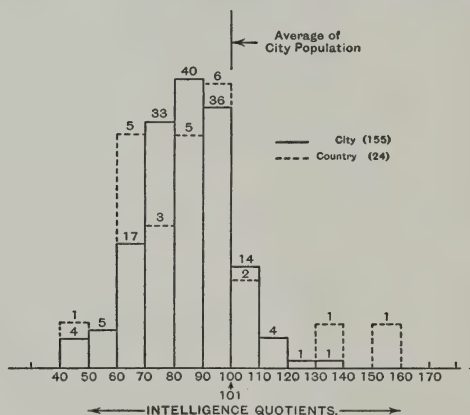


Fig. 2. Intelligence distribution of children of unemployed persons.

by some serious sociologists that the capacity to produce wealth is associated with inheritable mental attributes such as intelligence, but the facts of persistence of wealth in certain families may prove equally well an association of the capacity to seize property from others with inheritable or environmentally produced deficiency of character.

Nevertheless, it cannot be denied that the wealth of a community possessing a given quantity of raw material depends upon the quality and number of its inventive and organizing brains—and if inventive capacity is high enough, almost anything can become raw material. A decline in the numbers of high intelligence quotients must consequently mean a decline in the amount of goods and energy available for distribution in that community.

(c) In moral standards

The experiences of Child Guidance Clinics and the repetition in various parts of the world of the researches of Burt⁽¹⁾ and Healy⁽¹⁰⁾ indicate that delinquent children are on an average of significantly lower intelligence than non-delinquent children from the same social stratum.

Among the probable causes of this connexion seem to be: (i) The duller child does not so easily foresee the consequences of his actions or the likelihood of detection. (ii) Low intelligence is associated with suggestibility to evil counsels. (iii) In a complex environment the less able child is less successful in finding the normal amount of self-expression for psychic energy. He is more frequently frustrated or exploited by others. He is, in general, more hard pressed by economic restriction. In short, he has to handle a greater load of suppression. (iv) Whilst he suffers from this greater curtailment of direct instinctive expression, he is at the same time less able to achieve satisfaction in alternative indirect expression; for adequate intelligence is a condition of satisfaction in sublimations through art, music, science, social service and the activities approved by civilization. (v) Since he is of low intelligence there is a statistical probability that he has been brought up by parents of low intelligence, who have followed inconsistent and short-sighted policies in character education.

The relative importance of each of these processes would be better understood if there were evidence as to whether delinquency is associated with absolute low intelligence, or merely with intelligence which is low relative to the average for the group.

If the latter, then a decrease in average intelligence, the scatter remaining constant, would not result in increased delinquency, for the tendency to delinquency is in proportion to the maladaptation of the lower ranges to the culture standards of the pervasive average. This argument, however, overlooks the fact, long pointed out by such psychologists as McDougall, that the crust of custom and tradition has a persistent life of its own, beyond that of the individuals creating the custom. Though the average of intelligence may fall, therefore, the standards of behaviour, discrimination and sublimation can be expected to persist for a generation or more, with only sporadic patches of decrepitude. It is the discrepancy between the level of this persisting culture and that of the mental capacity of the new generation of living individuals which is crucial to the development of delinquency.

Under delinquency we commonly think solely of delinquency against society, yet there are some forms of behaviour damaging to the individual and society which might be called delinquency against nature, for they arise from a mishandling of human dealings with nature. Such behaviour extends from the small child hurting himself because he refuses to recognize the physical properties of a chair to a society injuring itself through reckless use of an obviously limited supply of natural resources. In child-guidance work it is often very strikingly illustrated that the child whose stupid reactions bring him into excessive conflict with his fellows is also excessively scarred through accidents arising from a stupid handling of natural objects. This delinquency against nature involves the group as well as the individual; for the group suffers from the individual's excess of activity over judgment.

From such considerations it seems reasonable to conclude that delinquency as it is commonly understood—'delinquency against society'—is a function of the difference between the individual's intelligence and that of the average of the society to whose standards he is constrained; whereas delinquency in the wider sense—'delinquency against nature', resulting in any kind of damage to society and the individual—is a function of the absolute level of intelligence.

Owing to that persistence of culture habits described above, however, the fall in average intelligence which we are discussing would, in fact, result in an increase of both types of anti-social action. It would be interesting to know what part of the present increase in juvenile delinquency is attributable to intelligence distribution changes.

Throughout it has been the plan of this enquiry to confine the investigation to the direct psychological effects. Nevertheless it cannot be overlooked that these would often be powerfully accentuated or modified by simultaneous effects operating in other fields, e.g. the increase in economic divergence discussed above would increase the provocation to delinquency. The possible reciprocations of primary consequences, resulting in consequences of a second order, are too numerous to study here; but in the matter now being discussed there is a point of such interest that it would seem to justify departing from the condition that other things are equal, and following up the result of another social variable varying along with intelligence.

Above we have supposed that the demands of culture will persist at their present level. Although culture habits in their deeper levels have considerable persistence, there are times, e.g. at the birth of some new religious impulse, or the impact of one culture upon another, when few

things can change more rapidly. It is possible, therefore, that instead of persisting the present restraints will either slacken, in response to the fall of intelligence, or that an attempt at over-compensation will be made by tightening up inhibitions to a hitherto unobtained level.

Before looking at the consequences of these alternatives, it is necessary to consider the ways in which instinctive expression is modified by culture: Generally the original biological goal of hunger, sex, gregariousness, etc., is maintained, being reached, however, by a longer circuit. Instead of hunting his food a man may earn it by working as a clerk. This involves a certain psychological strain and demands, generally, more intelligence than the original mode of behaviour. But instincts may even become what Freud has called 'aim inhibited', i.e. sublimated to some goal resembling but not as satisfying as the original biological goal. This renunciation demands 'plasticity' in the nervous system, a thing about which we know little, but generally it also requires intelligence, for the substitutes offered are generally of a complex kind.

Neither of these instinct changes is, in itself, better carried out by reason of good intelligence (unless it should be proved that 'plasticity' correlates with intelligence). Pascal is said to have responded to a disappointment in love by becoming increasingly entranced with the higher mathematics, and this sublimation is clearly not possible without such an intelligence as would make mathematics fascinating; but a feeble-minded person might respond equally well to the same situation, as in cases I have observed, by an interest in painting walls or smashing stones with a hammer. Similarly a man who doesn't get his food by the enjoyment of hunting may earn it, if he has a low I.Q., by turning a crank, with no more long-circuiting than occurs in the individual with a higher I.Q. who does the work of a clerk.

The chief reason, therefore, why the person of greater capacity is less delinquent and more able to tolerate either long-circuiting of instinct or its sublimation must be that civilization offers more opportunities for long-circuiting and sublimation at higher levels than at lower levels of complexity.

For all individuals some of the time, and some individuals all of the time, the psychological strain of these instinct modifications—which I shall call 'deflection strain'—must approach the limit of toleration, so that delinquency is threatened. This problem has been dealt with from one aspect by Freud in *Civilization and its Discontents*. From the present aspect we see that any marked fall of average intelligence with the same

standard of culture is going to increase 'deflection strain' to an explosive pitch.

In individual psychology it frequently happens that free-floating emotion from unexpressed unconscious sources irrationally fastens itself upon some chance external object, or that unconscious attitudes are 'projected' upon innocent objects. The irritation engendered by the frustrations of 'deflection strain' may well be one of the main drives for acts of war, and for the projection of hostile attitudes upon foreign countries. A fall of intelligence in a civilization which retains its complexity in spite of the less adequate mentalities of its bearers may, therefore, be associated with an increased readiness to action which relieves the tension of 'deflection strain' by war.

The first solution of the 'deflection strain' arising from intelligence decline which is likely to appeal to a community conscious of its cause, would be a systematized relaxation of moral standards, permitting more direct instinctive satisfactions and harmless and culturally valueless long-circuitings. The widespread creation of easy amusements in the cinema, the radio and the best-seller novel show that this is going on to-day, and these institutions might cater for a fall in average intelligence in such a way as to handle 'deflection strain' without allowing any increase in delinquency. Economic measures might do the same, for 'deflection strain' is greater for low than for high intelligences under conditions of material deprivation. The substitution of lower for higher recreational values is, however, a moral loss to the community in the fullest sense of morality.

There remains the alternative of the community being provoked, as it were, by the threatened decline into an actual increase of restraints and the imposition of higher standards, such as might occur in a strongly religious society. Granted sterner inhibition the limits of deflection strain would then be indicated not by an increase in delinquency but by a mounting incidence of suicides, melancholiacs and perhaps neurotics generally. At the same time there would be a loss of expressiveness and *joie-de-vivre* throughout the community, with resultant changes in the creativity and in the general type of culture. To maintain the same percentage of delinquency with a falling intelligence average, therefore, it seems that the community must either invent substitute satisfactions at a low level or revert to Spartan or Puritanical standards of suppression.

(d) In political and social life

We are confining consideration in the main to consequences of a fall of intelligence, but the kind of change actually foreshadowed in the birth-rate survey described almost certainly includes also an increase in the standard deviation, principally through a relatively large increase in the 70-80 i.q. range remote from the present mode.

The political consequences of such a change may be expected to arise directly through the increasing mental distance between individuals, and indirectly through changes in economic status brought about by the change. Outstanding among the latter would be the development of a larger 'social problem group' or at least of a group supported, supervised and patronized by extensive state social welfare work. Even if, through the supersession of *laissez-faire* by control of earnings, increased economic divergence were prevented, the direct 'distancing' effect would still persist: people may have uniform wage-rates and yet diverge greatly in social life and habits through differences in complexity of occupation, prestige and the presence or absence of intellectual interests. Any increase in the standard deviation is inimical to that human solidarity and potential equality of prestige which is essential to democracy.

Experimental evidence in the political field is slight but consistent. In three American Universities⁽¹⁴⁾ where the students had already been tested for intelligence, a questionnaire permitting a rating on a Radical-Conservative scale and in which the political views were given adequate place, showed a significant relation between higher intelligence and more radical views. Other researches^(8,11) confirm this relationship. Terman⁽¹⁶⁾, having worked out from childhood records the i.q.'s of eminent men in political life, afterwards sorted them into radical and conservative groups. The latter had the lower average intelligence.

Psychological analysis suggests that the above is not the whole story. To see all sides of a question, to make a discriminating and qualified, rather than an all-or-nothing response, requires higher intelligence. A fall of average mental capacity means an increase in the number of people capable of being led into extremist positions.

It is a commonplace in education that freedom can generally be safely given to a child in proportion to his intelligence. Newer systems of education, allowing the child considerable rights of self-determination, work admirably with gifted children; but at the other end of the scale, among border-line feeble-minded children in special classes, it is noticeable that children neither desire nor tolerate much individual freedom. It is

as if, intuitively, or through experience, they realize that the greatest happiness of all concerned depends upon their being given rigid rules of conduct prescribing behaviour even to the extent of most detailed traditions and taboos. In social behaviour, as in arithmetic, they cannot safely be left to proceed far on their own.

Though the above relation of conservatism with lower intelligence has been described in the political field, the failure to appreciate reform or to desire the freedom to which reform will lead is shown equally, by the questionnaire study (8), to hold for progressiveness in the social field. Reformers during a generation suffering a falling average of intelligence will consequently experience greater difficulty in finding support for intelligent modification of obsolete, unadjusted habits and a slowing down of social change may therefore be expected.

The above considerations combined suggest the probability of political changes of a complex kind. They include lack of cohesion between social groups, together with changes of mental attitude such as might lead to the dissolution of the ideal of democracy; a decrease of the tempo of liberal progressiveness, an increase in those who pin their faith to simplified extremist formulae; and a hardening of rigid disciplines which preclude individual freedom.

(e) *In general culture*

There remain for discussion certain effects of a more general kind on the mental life of the group as a whole.

Among people exposed to much the same cultural influences, it is surprising how highly the amount of general information at the command of any individual is dependent upon mental capacity. Correlations of 'g' with size of vocabulary, for example, are commonly as high as 0.8.

The smooth and efficient running of social life in a civilization of a complex kind, having many specialized institutions, requires a reasonably high level of general knowledge in all its members. Civilization is, after all, largely a body of knowledge—words, for example, do not live in dictionaries, but in the minds of writers—and people who cannot heed important parts of that background certainly cannot acquire the appropriate attitudes. Furthermore, any specialized question, affecting the life of the community as a whole, must be able to impinge upon a sufficient number and variety of alert people to ensure that the reaction is such as to be in the interests of the community as a whole. A decline in average intelligence means a shrinkage of the field of general knowledge which each person is capable of absorbing, and consequently a reduction

in the number of people aware of anything but the simplest issues within the ambit of their daily lives. This must lead to an inertness of society and a dangerous slowness or lack of integration in responding to political and social dangers.

This problem has already been studied experimentally in the previous section with regard to politics; we may here look at it in a more general way. Any new idea appearing in a community can be reacted to by those who understand it either in a friendly or a hostile fashion. But the people who do not understand it will usually treat it in a hostile fashion. Every idea must have, according to its complexity, a certain percentage of the community capable of appreciating it. It can percolate downwards very rapidly so far but, reaching the larger stratum of people who understand it with difficulty, it moves very slowly and must depend for its progress upon emotional pressure and extraneous historical factors.

There is thus for every idea a certain I.Q. which may be called its 'demostatic level', down to which it may be fully understood, and an I.Q. stretch below that over which it has some lesser influence, which may be called its 'percolation range'.

Thus in the field of literary art Leavis⁽¹²⁾ complains that "the rate at which *cultural* news penetrates is surprisingly slow...considering the elaborate machinery for disseminating such news which civilization possesses", and continues "the Book Clubs are instruments not for improving taste but for standardizing it at the middlebrow level, thus preventing the natural progression of taste that in the later eighteenth century, for instance, was assisted". The phenomenon would be explicable on the present hypothesis on the grounds that in the eighteenth century 'taste' had still not penetrated to its demostatic level, whereas to-day it has, and the maligned Book Clubs are only stabilizing each variety of taste at its most natural and stable level. The naïve supposition that progress of ideas downwards through society is continuous though slow, arises from the observation of slow movement in the 'percolation range'; but in fact no idea can get beyond its percolation range, and may be proceeding to an arrest at an I.Q. level which must be quite high in certain instances, e.g. the Special Theory of Relativity.

The vigour with which mental energy invests an idea or course of action is a function of the simplicity of the latter. Corresponding to the demostatic level and percolation range in society there is for each individual, in the range of ideas which assails him, an upper limit of easy comprehension ending in a point beyond which no further complexity can be handled at all. To the range beyond this point he can generally

apply nothing but a passive hostility; to the middle range of dim comprehension he can give only weak support or criticism; to the lowest range he can give the full weight of his emotional life. Consequently the amount of energy available in society for any given idea must depend on the demostatic level for that idea and the percentage of people in the given community above that I.Q. level, plus a value obtained by multiplying the numbers in each level of the percolation range by a fraction which diminishes from one to zero in passing from the top to the bottom of the percolation range. This we may call the 'psychic energy investment' of the idea, and it will vary from community to community according to the average level of I.Q. and the distribution of I.Q.

Between any two minds in the same culture there is bound to be more or less overlap in knowledge, interests and sentiments. This we may call 'mental overlap', and upon its extent depends the amount of social cohesion in any purpose which may attract the energy of society. Good mental overlap does not ensure common purpose, but it is the necessary fruitful soil of common sympathy in which the latter may grow. A reduction of average mental capacity means a reduction of mental overlap in all minds and a failure of any mental overlap whatever in the socially, geographically and occupationally more remote minds; so that social cohesion, and the size of the group in which successful cohesion can be maintained, are diminished. Reduction of mental overlap arising from that diminution in the radius of individual minds produced by the substitution of lower for higher I.Q.'s thus has two major consequences:

- (1) The impairment of social cohesion.
- (2) Reduction in the intensity of cultural life, which is the sum of these overlaps, or, otherwise expressed, of the total psychic energy investment of a given set of ideas—the ideas which give form to that culture.

A similar process must take place within the limited fields of the special sciences and arts. The inability of what we regard as the intelligent person to cope with the increased complexity of modern culture has been met by accepting the device of specialization. Versatile minds, capable of taking all knowledge for their province, are far rarer to-day than in Elizabethan times; indeed it has begun to be considered presumption to attempt proficiency in more than one field of learning. Yet co-ordinating minds are always urgently needed, and the progress of one science often depends upon its being brought into relation with another. Either a further increase in accumulated but undigested

information, or a decline of mental capacity in the workers serving those sciences, or both, would have the effect of bringing major progress to a halt and of preventing a synthesis of sciences either for the practical good of the community or for the attainment of truer philosophical perspectives.

The relation of intelligence to the type of interests, particularly social and recreational interests, must next be considered, for the latter react subtly yet strongly on the individual personality as a whole, and consequently upon the effectiveness of the individual as a member of society.

In a research in which children's preferences for various tests were studied, the present writer found that the obtained order of preference was practically the reverse of the order of 'g' saturation(3). Above a certain modest intelligence demand the occupations were most liked which demanded least use of intelligence. The same is found with school subjects; the brilliant child may prefer English or Mathematics, both highly 'g' saturated, but the dull child takes a natural interest principally in handwork, games and simple repetitive tasks.

In the recent research on popular reading habits, mentioned above(12), the literary student scornfully compares the standard of the accepted newspaper of 1850 with that of to-day, adding some scathing comments on fifty years of free and compulsory education. But, as indicated in the discussion on demostatic level, the difference is far more simply explained as being due to the increased social range in the reading public, necessarily accompanied by an increased percentage of readers from the lower ranges of the intelligence distribution curve, a change equivalent to a decline in the average mental capacity of the reading public. Even so the decline in standards might not have occurred without the newspaper magnates' devotion to "supplying the public with what it wants". But had this not been done on commercial grounds it would have been done on grounds of freedom; indeed in most fields of recreation it is in any case impossible to prevent people doing the things for which their level of intelligence best fits them. It is a short-lived mistake to force Aldous Huxley on people who want all-in wrestling, or Beethoven on those who can only enjoy the crudest of cinema drama.

When coercion by schoolmaster and highbrow ceases, as it is ceasing, because of its fruitlessness, a more subtle kind of coercion in the reverse direction will have to be taken into account. Comparing the tone of the Press in America and in this country, H. G. Wells has cautioned us that there are no more fools in America than in Great Britain. It is only that they are more vocal there.

Once free expression is conceded it is clear that the type which constitutes the mode of the intelligence distribution is going to become the most loudly expressed. Owing to the emotional force of the gregarious instinct and the wear and tear which the individual constantly suffers in trying to detach his reactions from those of the dominant average, the modes of expression of the more intelligent are always constrained towards those of that average. Without a sharper splitting of the social life of intellectual classes this is inevitable, and since intellectual classification is not the same as social classification, being less organized, only societies based on special interests keep the rarer interests alive.

These psychological forces quickly bring in their train material and economic associates. Thus when publishing becomes a purely commercial proposition, no publisher is likely to take on a 'highbrow' author with small circulation when he can employ a 'best-seller' author. The more widely used commodity actively pushes that desired by a few people off the market, whether that minority happens to be merely eccentric or culturally important.

Some alienation of the recreational and cultural interests of various classes may well occur if we continue to facilitate, through equalization of opportunity, the sorting out of classes according to intelligence, and if the natural variability of intelligence is at the same time increased through the present birth-rate trends. In any case the fall in the I.Q. level of the mode itself must result in a significant impoverishment and stultification of the cultural life of the average and the superior.

Finally we have to consider a general change towards which many primary and secondary changes are working. We have seen that there must be some decline in the cultural level attained by education, and some arrest in economic progress. To these effects must be added a decrease within the community as a whole of qualities of foresight and resistance to suggestibility, for deficiency in these is particularly characteristic of persons of defective intelligence (15).¹

Increasing deflection strain not alleviated by any deliberate or accidental lowering of cultural demands may be reacted to, as we have seen, either by individual delinquency or war. But since all the above changes in education and economics, the diminished foresight and increased suggestibility, are such as to produce minor or major internal or external sufferings or calamities, the course of history may well lead

¹ An academic acquaintance, visiting the dentist remarked, "seeing a dentist before a tooth hurts is an act of pure intelligence", and though foresight is a conative rather than a cognitive quality it undoubtedly correlates with intelligence.

to adjustment by way of increased inhibition rather than by way of appropriate reduction of deflection strain.

In that case two responses are possible; either the traditional reaction to frustration by religious emotion and observance or the newer expression of thwartings through the phantasy of the novel and the cinema. This phantasy expression is to-day reaching tremendous dimensions, and we have probably not seen the end of its development or begun to appreciate its damaging effects on 'reality thinking' habits concerned in other spheres of life.

As regards the increase in a religious type of adaptation, it is clear that the trend of rationalist education may be powerfully against this, but the increased conservatism, suggestibility and limitation of mental range are in its favour. And rationalist notions, at least constructive rationalist notions attempting to give a philosophic conception of life in keeping with all the established findings of modern science, are bound quickly to reach their demostatic level, whereas the cruder symbolisms and approximations to the same truths contained in religion will readily acquire, owing to the bulge in the lower ranges of the I.Q. distribution curve, a far higher psychic investment energy. The imponderables, especially in this final calculation, are too many to justify the statement that a back-to-religion movement in the conventional sense of religion is probable, but the rough draft of a formula for the social response to an I.Q. decline can at least be given, and one possible solution of it is clearly an increase in religious forms of self-expression.

V. SUMMARY

This thesis, elaborated partly on experimental and partly on analytical grounds, indicates that the probable consequences of a downward shift in the intelligence distribution curve are as follows:

1. A fall in academic standards in the schools.
2. A change in the curriculum of schools towards less abstract and generalized studies.
3. An increased cost of education.
4. Increased unemployment in the less-skilled occupations.
5. Decrease in the average real earning capacity of the community as a whole.
6. A rise in the frequency of delinquency (unless there is a deliberate lowering of moral standards) or/and proneness to aggression between nations.

7. Alternatively, if inhibitory forces prevail, either (a) an increase in the social equipment provided for phantasy compensations, or (b) an increase in religious expression.

8. An increase in the percentage of people adopting extreme or uncompromising political view-points, together with the growth of a generally conservative position.

9. An increased retardation in the percolation of 'cultural news', together with a lowering of the intensity of cultural life and a diminution in the rate of scientific discoveries and other specialized advances.

10. A shift of cultural and recreational interests to cruder tastes and forms of expression, together with an increased divergence of interest between different groups and a greater domination by the average.

11. A check to the growth of social and political freedom and a reversion to a more detailed prescription of individual behaviour.

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THE INFLUENCE OF UNIVARIATE SELECTION ON THE FACTORIAL ANALYSIS OF ABILITY¹

BY GODFREY H. THOMSON

Moray House, University of Edinburgh

- I. *Statement of the problem; and some formulae* (pp. 451–453).
- II. *Minimal rank unchanged by selection* (pp. 453–455).
- III. *The new communalities* (pp. 455–456).
- IV. *Two numerical examples* (pp. 456–458).
- V. *Conclusion* (p. 458).
- References* (p. 459).

I. STATEMENT OF THE PROBLEM; AND SOME FORMULAE

By selection in the present paper is meant selection among the persons to whom tests are applied. A parallel article could be written on the result of selection among the tests given. There is indeed a triple parallelism between persons, tests, and factors which accounts for curious similarities in apparently different formulae(1). To avoid complications, however, I am restricting the present paper entirely to the influence which sampling the *population* has on the factorial analysis of a battery of tests.

Factorial analyses, by whatever method they are conducted, depend upon a matrix of correlations or of covariances: and these correlations or covariances vary with the sample of persons to whom the tests are administered. Any factorial analysis of a test therefore depends not only upon the other tests with which it is brigaded in a battery, but also upon the population of persons from whose scores the correlations are calculated. It is not a fixed but a fluid thing. Similarly the estimate which will be made of a man's factors will depend not only on his scores in certain tests, but on the particular sample of other persons from whose scores the coefficients used in estimating his factors are calculated.

The influence of selection on variance and correlation was first considered by Karl Pearson(2) and a summary of his formulae, with some examples by way of illustration, is given in Ch. VII of *The Essentials of Mental Measurement*(4). I have found in teaching however that it is a

¹ Part of a study made possible by a grant from the Carnegie Corporation of New York.

considerable simplification to divest those formulae of their trigonometrical dress, as follows:

Let $x_1, x_2, x_3, \dots$ be a number of variables (test-scores) which in a certain population have standard deviations $\Sigma_1, \Sigma_2, \Sigma_3, \dots$ and correlations $R_{12}, R_{13}, R_{23}, \dots$. Now let a selection of the population be made with regard to their scores in x_1 , such that the standard deviation of x_1 is reduced from Σ_1 to σ_1 (on the extent to which the formulae are true even if distributions are not normal, see reference (3)) and call

$$\sigma_1/\Sigma_1 = p_1.$$

If the standard deviation of x_1 is shrunk very much, p_1 will become small, and

$$q_1 = \sqrt{1 - p_1^2}$$

will be large: let us call q_1 the 'shrinkage' of x_1 .

A direct selection of the population has thus been made with regard to their scores in x_1 . As a consequence of the intercorrelations of the variables, shrinkages will also occur, though of less violence, in the standard deviations of the other variables, and changes in all the correlations. The shrinkages in the other variables can be calculated by the formulae

$$q_i = q_1 R_{1i}, \quad (1)$$

which shows that if x_1 and x_i are highly correlated the shrinkage of x_i will closely follow that of x_1 , while if they are uncorrelated x_i will not shrink at all. The correlations of the directly selected variable with the other variables will change from R_{1i} to

$$r_{1i} = \frac{p_1}{p_i} R_{1i} = \frac{p_1 q_i}{q_1 p_i}, \quad (2)$$

that is, they will be reduced, for the shrinkage q_i is less than q_1 , and p_i therefore greater than p_1 .

The correlations of the other variables among themselves will change from R_{ij} to

$$r_{ij} = \frac{R_{ij} - q_i q_j}{p_i p_j}, \quad (3)$$

which reduces to formula (2) if either variable is the directly selected one. It is also often useful in the form

$$R_{ij} = p_i p_j r_{ij} + q_i q_j, \quad (3a)$$

in which it gives the value of R_{ij} in the parent population when r_{ij} in the

sample is known, and the two shrinkages. If the latter are equal it becomes $R = p^2r + q^2$ which is readily seen to be the Otis-Kelley formula

$$\frac{1-R}{1-r} = \frac{\sigma^2}{\Sigma^2}.$$

II. MINIMAL RANK UNCHANGED BY SELECTION

Except for some simplifications in notation the formulae of the preceding section are not new. The sections which now follow however appear to be new.

With a number t of tests in the whole population the complete matrix upon which factorial analyses are based is

$$R_1 = [R_{ij}]_1 = \begin{bmatrix} 1 & R_{12} & R_{13} & \dots & R_{1t} \\ R_{12} & 1 & R_{23} & \dots & R_{2t} \\ R_{13} & R_{23} & 1 & \dots & R_{3t} \\ \cdot & \cdot & \cdot & \dots & \cdot \\ R_{1t} & R_{2t} & R_{3t} & \dots & 1 \end{bmatrix},$$

where the subscript in R_1 or $[R_{ij}]_1$ indicates that each cell of the leading diagonal is occupied by unity.

In a population directly selected so as to shrink the variable x_1 the matrix will become

$$R_1^* = [r_{ij}]_1 = \begin{bmatrix} 1 & \frac{p_1 R_{12}}{p_2} & \frac{p_1 R_{13}}{p_3} & \dots & \frac{p_1 R_{1t}}{p_t} \\ \frac{p_1 R_{12}}{p_2} & 1 & \frac{R_{23} - q_2 q_3}{p_2 p_3} & \dots & \frac{R_{2t} - q_2 q_t}{p_2 p_t} \\ \frac{p_1 R_{13}}{p_3} & \frac{R_{23} - q_2 q_3}{p_2 p_3} & 1 & \dots & \frac{R_{3t} - q_3 q_t}{p_3 p_t} \\ \cdot & \cdot & \cdot & \dots & \cdot \\ \frac{p_1 R_{1t}}{p_t} & \frac{R_{2t} - q_2 q_t}{p_2 p_t} & \frac{R_{3t} - q_3 q_t}{p_3 p_t} & \dots & 1 \end{bmatrix}.$$

The factorial analysis will be changed as a consequence, and the equations representing the composition of the tests in terms of factors will alter. The minimum rank, however, to which the matrix can be reduced by reducing the unit leading elements to suitable proper fractions called communalities, has not altered; so that if the Thurstone method of analysis had been used, which depends upon that rank, the number of

common factors in the tests would remain the same, but their loadings or saturations would have changed.

To see that the minimal rank has not changed, consider that it depends on those minors of R_1 (in the population) or R_1^* (in the sample) which do not trespass on the leading diagonal. All such minors which vanish in R_1 also vanish in R_1^* . Consider one of them of order (say) three, and let it be in the first instance one which includes a portion of the column belonging to x_1 , say

$$\begin{vmatrix} \frac{p_1 R_{12}}{p_2} & \frac{R_{25} - q_2 q_5}{p_2 p_5} & \frac{R_{26} - q_2 q_6}{p_2 p_6} \\ \frac{p_1 R_{13}}{p_3} & \frac{R_{35} - q_3 q_5}{p_3 p_5} & \frac{R_{36} - q_3 q_6}{p_3 p_6} \\ \frac{p_1 R_{14}}{p_4} & \frac{R_{45} - q_4 q_5}{p_4 p_5} & \frac{R_{46} - q_4 q_6}{p_4 p_6} \end{vmatrix}.$$

This determinant equals, since $q_2 = q_1 R_{12}$, etc.,

$$\begin{aligned} & \frac{p_1}{p_2 p_3 p_4 p_5 p_6} \begin{vmatrix} R_{12} & R_{25} - q_1 q_5 R_{12} & R_{26} - q_1 q_6 R_{12} \\ R_{13} & R_{35} - q_1 q_5 R_{13} & R_{36} - q_1 q_6 R_{13} \\ R_{14} & R_{45} - q_1 q_5 R_{14} & R_{46} - q_1 q_6 R_{14} \end{vmatrix} \\ &= \frac{p_1}{p_2 p_3 p_4 p_5 p_6} \begin{vmatrix} R_{12} & R_{25} & R_{26} \\ R_{13} & R_{35} & R_{36} \\ R_{14} & R_{45} & R_{46} \end{vmatrix}. \end{aligned}$$

That is to say, this minor of R_1^* is equal to the corresponding minor of R_1 multiplied by a constant depending upon the shrinkages. In particular, this minor ($r_{12} r_{35} r_{46}$) is zero if ($R_{12} R_{35} R_{46}$) is zero. Consider next a minor which does not include any portion of the first column or row, say ($r_{25} r_{36} r_{47}$). Its determinant is found by a similar method to be

$$\frac{1}{p_2 p_3 p_4 p_5 p_6 p_7} \begin{vmatrix} R_{25} - q_1 q_5 R_{12} & R_{26} - q_1 q_6 R_{12} & R_{27} - q_1 q_7 R_{12} \\ R_{35} - q_1 q_5 R_{13} & R_{36} - q_1 q_6 R_{13} & R_{37} - q_1 q_7 R_{13} \\ R_{45} - q_1 q_5 R_{14} & R_{46} - q_1 q_6 R_{14} & R_{47} - q_1 q_7 R_{14} \end{vmatrix}$$

and this also equals zero if the rank of the matrix

$$\begin{bmatrix} R_{12} & R_{25} & R_{26} & R_{27} \\ R_{13} & R_{35} & R_{36} & R_{37} \\ R_{14} & R_{45} & R_{46} & R_{47} \end{bmatrix}$$

equals two. If therefore the rank of R_1 is reducible to two, so also is the rank of R_1^* which is produced from it by selection in any variate. And the same is true for rank r , for the above argument can clearly be written out for any size of minor. The minimal or Thurstone rank is not changed by selection, the 'reduced' matrices R and R^* (that is, the matrices with communalities in the leading diagonal) are of the same rank.

III. THE NEW COMMUNALITIES

The new communalities can be found in terms of the old by similar considerations. Let the communalities of the reduced matrix R be H_1^2 , H_2^2 , H_3^2 , ..., etc., and for illustrative purposes suppose the rank is two. The method however is clearly quite general. Then we have, for example,

$$\begin{vmatrix} H_1^2 & R_{14} & R_{15} \\ R_{12} & R_{24} & R_{25} \\ R_{13} & R_{34} & R_{35} \end{vmatrix} = 0.$$

The new communalities h_1^2 , h_2^2 , h_3^2 , ..., etc., in R^* after selection are such that

$$\begin{vmatrix} h_1^2 & r_{14} & r_{15} \\ r_{12} & r_{24} & r_{25} \\ r_{13} & r_{34} & r_{35} \end{vmatrix} = 0,$$

that is

$$\begin{vmatrix} h_1^2 & \frac{p_1^2}{p_1 p_4} R_{14} & \frac{p_1^2}{p_1 p_5} R_{15} \\ \frac{p_1^2}{p_1 p_2} R_{12} & \frac{R_{24} - q_2 q_4}{p_2 p_4} & \frac{R_{25} - q_2 q_5}{p_2 p_5} \\ \frac{p_1^2}{p_1 p_3} R_{13} & \frac{R_{34} - q_3 q_4}{p_3 p_4} & \frac{R_{35} - q_3 q_5}{p_3 p_5} \end{vmatrix} = 0,$$

that is

$$\frac{p_1^2}{p_2 p_3 p_4 p_5 k} \begin{vmatrix} k h_1^2 / p_1^2 & k R_{14} & k R_{15} \\ R_{12} & R_{24} - q_1 q_4 R_{12} & R_{25} - q_1 q_5 R_{12} \\ R_{13} & R_{34} - q_1 q_4 R_{13} & R_{35} - q_1 q_5 R_{13} \end{vmatrix} = 0,$$

where k is a constant. This will be so if

$$\left. \begin{aligned} k h_1^2 / p_1^2 &= H_1^2 \\ k R_{14} &= R_{14} - q_1 q_4 H_1^2 \end{aligned} \right\},$$

and

$$\text{that is} \quad h_1^2 = \frac{p_1^2 H_1^2}{1 - q_1^2 H_1^2}. \quad (4)$$

The other reduced communalities are more easily found by a similar method, and we arrive at the result for them

$$h_i^2 = \frac{H_i^2 - q_i^2}{p_i^2} \quad (i = 2, 3, 4, \dots). \quad (5)$$

Dr Walter Ledermann, on reading the MS. of this paper, has pointed out to me that formula (3) covers this case too if we write $R_{ii} = H_i^2$ and $r_{ii} = h_i^2$; and that by a slight change in notation formula (4) can also be included, and one algebraic expression made to represent the relation of every element in R^* to the corresponding element in R . I leave him to develop this and other points in a future paper of his own.

Formula (5) can also be written

$$h_i^2 = H_i^2 - \frac{q_i^2}{p_i^2} S_i^2 \quad (H_i^2 + S_i^2 = 1), \quad (5a)$$

which shows us that selection in x_1 has decreased the communality and increased the specificity of each of the other variables by q_i^2/p_i^2 of the original specificity.

In this section I have only written out the proof for the case of rank 2, and for a certain minor of order 3. But the subscripts, of the particular minor chosen, disappear from the final formulae and it is clear that a similar argument could be written out for any minor and for any rank, and that formula (4) for the directly selected variable and formula (5) for the others are general expressions for the reduction in communality and consequent increase in specificity.

IV. TWO NUMERICAL EXAMPLES

I give next two numerical examples to illustrate these formulae:

Example 1. Let a certain matrix of correlations, with minimal communalities inserted, be

$$R = \begin{bmatrix} (0.81) & 0.72 & 0.63 & 0.54 & 0.45 \\ 0.72 & (0.64) & 0.56 & 0.48 & 0.40 \\ 0.63 & 0.56 & (0.49) & 0.42 & 0.35 \\ 0.54 & 0.48 & 0.42 & (0.36) & 0.30 \\ 0.45 & 0.40 & 0.35 & 0.30 & (0.25) \end{bmatrix}.$$

This matrix is of rank one, and the tests have the Spearman analysis

$$x_1 = 0.9g + \sqrt{0.19} s_1,$$

$$x_2 = 0.8g + \sqrt{0.36} s_2,$$

$$x_3 = 0.7g + \sqrt{0.51} s_3,$$

$$x_4 = 0.6g + \sqrt{0.64} s_4,$$

$$x_5 = 0.5g + \sqrt{0.75} s_5.$$

Now let the persons to whom these tests are given be selected so that the standard deviation of their scores in the test x_1 is only $p_1 = 0.6$ of what it was, the shrinkage being $q_1 = \sqrt{(1 - p_1^2)} = 0.8$. Then by formula (1) we find the other shrinkages to be

for	$i = 1$	2	3	4	5
	$q_i = 0.8$	0.576	0.504	0.432	0.360
and	$p_i = 0.6$	0.817	0.864	0.902	0.933,

and by means of formulae (2), (3), (4) and (5) we can write down the matrix of correlations in the new set of persons, namely

$$R^* = \begin{bmatrix} (0.604) & 0.528 & 0.437 & 0.359 & 0.289 \\ 0.528 & (0.461) & 0.382 & 0.313 & 0.253 \\ 0.437 & 0.382 & (0.315) & 0.259 & 0.210 \\ 0.359 & 0.313 & 0.259 & (0.213) & 0.171 \\ 0.289 & 0.253 & 0.210 & 0.171 & (0.139) \end{bmatrix}$$

which we see to be also hierarchical and to correspond to an analysis of the tests

$$x_1 = 0.777g + 0.629 s_1,$$

$$x_2 = 0.679g + 0.734 s_2,$$

$$x_3 = 0.561g + 0.828 s_3,$$

$$x_4 = 0.462g + 0.877 s_4,$$

$$x_5 = 0.373g + 0.928 s_5,$$

the saturations of g having all sunk, and the specifics having increased in importance.

Example 2. As a second example we take one where the matrix R is of rank two. Let the loadings of two factors in six tests, and the communalities and specificities, be as follows:

	x_1	x_2	x_3	x_4	x_5	x_6
g_1	0.7	0.6	0.5	0.4	0.3	0.2
g_2	0.4	0.1	0.6	0.2	0.5	0.3
H^2	0.65	0.37	0.61	0.20	0.34	0.13
S^2	0.35	0.63	0.39	0.80	0.66	0.87

Their matrix R can then be written down. Let there now be a selection so as to reduce the standard deviation of x_1 to 0.6 of its former value. We then calculate

p	0.6	0.930	0.882	0.958	0.945	0.978
q	0.8	0.368	0.472	0.288	0.328	0.208

and can write down the matrix R^* . We find that the communalities have decreased, and the specificities increased, to

h^2	0.401	0.271	0.498	0.127	0.261	0.091
s^2	0.599	0.729	0.502	0.873	0.739	0.909

The rank of R^* is still two, and a Thurstone analysis of it gives the unrotated loadings of the two factors as

I	0.622	0.418	0.681	0.345	0.461	0.279
II	-0.119	-0.311	0.184	-0.090	0.218	0.114

These can nearly—but not quite—be rotated into a form in which each loading is h_i/H_i of the loading in the unselected population. The exact relation between the loadings in population and sample will I think be dealt with by Dr Ledermann in his paper.

V. CONCLUSION

It should finally be noted that mathematically there is no distinction between sample and population, each is a distortion of the other. From a population with a certain scatter in x_1 a sample can be chosen with a larger as well as with a smaller standard deviation—in which case our formulae (1)–(5) will still hold if we remember that q_i^2 and $q_i q_j$ are now negative quantities. The one factorial analysis is neither better nor worse than the other. Each only has meaning with regard to the population to which it refers. Analyses are relative not only to the ‘set-up’ of the tests, but to the ‘set-up’ of the persons.

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CRITICAL NOTICE

BY J. T. MACCURDY

Naven: A Survey of the Problems suggested by a Composite Picture of the Culture of a New Guinea Tribe drawn from Three Points of View. By GREGORY BATESON. Cambridge University Press. 1936. Pp. xx+286, 28 plates and 7 diagrams. 18s. net.

Because it is a study of method in a field overlapping his own, this book will interest the psychologist even though he may feel incapable of evaluating it as an anthropological work. But it has in addition an importance for him, since it includes at least one hypothesis claimed to explain phenomena in the field of the social psychologist and suggested as a principle operating in the mental life of the individual as well. The title of the book is derived from a peculiar ceremonial behaviour on the part of the Iatmul people, which is described in its setting in the life of these people. Bateson, however, does not give us an arid collection of facts; these are used, rather, as a starting point for an inquiry as to how various institutions are related to one another and to the behaviour of the men and women who live under them.

Naven is a term given to a peculiar reversal, under special circumstances, of what we should call 'natural' attitudes and relationships. Men behave like women and vice versa, either wearing the clothes of the other sex, or elders abase themselves before their juniors. Since the people are given to buffoonery, many of the actions in celebration of *naven* are grotesquely undignified. The ceremonial relationship is between a *wau*¹ and his sister's child the *laua*. The ceremonies are performed in celebration of the acts and achievements of the *laua*. The extent and repetition of the *naven* acts depend on the importance of the *laua*'s achievement. The most important is homicide or assistance in homicide. This calls for elaborate *naven* which will be repeated with each killing. Next in importance comes the slaying of various animals, which may be celebrated only on the first occasion. In fact the first performance of almost any act significant in the Iatmul culture may be a signal for *naven*. But the occasion may not be an achievement at all. The *laua* may merely perform some action in the presence of his *wau* which points to their mutual relationship, whereupon the latter will assume his role. (So, in our culture, the kneeling of a Catholic calls forth a blessing from priest or bishop.) Again, if the *laua* indulges in too tedious boasting in the presence of his *wau*, the latter may indulge in *naven* acts and then the *laua* will have to give him presents. Finally, a change in the social status of the *laua* will move the *wau*; such changes are initiation or marriage, but the *naven* ceremonial in these cases has no part in the changes as such.

With an avowedly experimental aim Bateson sets himself to examine *naven* from various points of view, each of which represents the working hypothesis of some actual or hypothetical anthropological school.

The first of these is 'structure'. Structure is "a collective term for the coherent 'logical' scheme which may be constructed by the scientist, fitting together the various premises of the culture". 'Premise' must in turn be

¹ I.e. the mother's brother, the son's wife's brother and other relatives classed with them.

defined. It is "a generalized statement of a particular assumption or implication recognizable in a number of details of cultural behaviour". To put it less formally, in a structural study one puts one's self in the position of a hypothetical intellect inside the culture which scrutinizes a given element in order to demonstrate that it is consistent with other details of the culture.

It appears, for instance, that the *wau-lawa* relationship is not an isolated element in Iatmul culture but is a part of its structure. The anthropologist who was a structuralist would enlarge inquiries of this kind and would feel his task was complete when *naven* was demonstrated to be an integral element, a logically inherent feature of a culture having the premises his researches exposed. But to another worker this enlarges rather than solves a problem. This second anthropologist asks what use all these interrelated customs and beliefs are to the people who follow them. What role do they play in the integration of the society in question? This is the functional approach.

Bateson begins this part of his task with a sociological problem. The communities with which we are familiar are bound together by some kind of centralized authority, an authority vested in king or president and formulated as well in laws and binding conventions characteristic of the group. This is a *centripetal* kind of organization. So long as all the members recognize this focusing authority, the society sticks together. Difference from authority leads to subjugation, successful revolt or migration. In the latter two cases there is a change in chieftainship or ideals in the original group or there is a new group formed with a new cementing doctrine. In the *peripheral* type of society, however, there is no law and no law-giver, no statutory or conventional leader. Such a society tends to break up as a result of quarrels and the new groups are then like the original one.

The Iatmul people enjoy—or suffer from—a peripheral organization. They have, of course, some conventions and sanctions, but these are inconsistent and conflicting. The result is a highly individualistic society in which quarrels are common. Beginning between individuals these are apt to involve the clans, moieties and initiatory groups to which the disputants belong. These groupings are patrilineal. It follows that patrilineal organization tends towards peripheral disruption. Against this such an institution as kinship marriage would operate. Now there are some rules for such marriages but they are inconsistent and in practice by no means binding. Assuming that *naven* tends strongly towards the maintenance of affinal links, Bateson detects in it an influence cutting across the patrilineal system and therefore something which tends towards the cohesion of society.

Structurally and sociologically *naven* is thus accounted for, but other problems remain. Why *naven* and not some other institution that would equally well fulfil the roles so far described? There is, for instance, no explanation of the ridiculous buffoonery or histrionic exaggeration of the *wau* and no reason has been given for the *wau*'s apparent need of allegiance. Further the actions described are those of individuals, whose motives are, presumably, individual. Where does consistency of behaviour come from unless one is going to assume some group mind that coerces the unit mind? It is in the discussion of these, and cognate, problems that the most original part of Bateson's work appears. As aspects, or perhaps actual influences, in a culture he invokes principles which he labels as *Ethos*, *Schismogenesis* and *Eidos*.

Ethos is "the expression of a culturally standardized system of organization of the instincts and emotions of the individuals". It is something which we

have been accustomed to recognize in manners, national or class temperament, and scale of values. The ethos of a group will make its individual members 'play the game', be hospitable to, or hostile to, a stranger, take pleasures sadly or joke about misfortune, regard the old with respect, irritation or pity and so on. Among the Iatmul there is a great difference between the ethos of the men and that of the women. The men meet in a ceremonial house which to them has some kind of sanctity, but the orgies of an occidental revival meeting would be dignified in comparison with the behaviour of the Iatmul men in their debates which are the chief activities in the ceremonial house. In these debates the speakers are not debasing themselves in the presence of some mighty power but enhancing their individual prominence by boasting, bombast and buffoonery. The boasts are of knowledge, claimed to be esoteric, of their totemic ancestry proved through their knowledge, *inter alia*, of totemic names of people and things and of genealogies both actual and classificatory. The descriptions Bateson gives of these debates inevitably suggest to a European the boastings and tantrums of a spoiled child; the only element that is not 'childish' is the prodigious memory exhibited. Rather similar traits appear in the initiatory rites which reflect the bully's cruelty rather than a solemn ordeal which would produce or prove hardihood. The victim's struggles and cries are treated as a joke or a nuisance. If the men are childish the women are much more adult—as we should judge them. In place of individualistic, melodramatic swaggering there is co-operative good nature. The women go quietly about their business of food-getting, cooking, housekeeping and child-rearing. In private they are jolly, in public either retiring or proud when engaged in ceremonial display. Judging from Bateson's anecdotes it would seem that they know what they want and get it with the least possible fuss. The men pretend to despise the women but actually are dependent on them and a wife may not hesitate in depriving her recalcitrant husband of cooked food.

It is not difficult to see in the *naven* ceremonies a reflection of the histrionic male ethos but they are accounted for more fully by *schismogenesis*.

Schismogenesis is "a process of differentiation in the norms of individual behaviour resulting from cumulative interaction between individuals". Of this there are two types, complementary and symmetrical. Bateson describes the former in these words: "If, for example, one of the patterns of cultural behaviour, considered appropriate in the individual *A*, is culturally labelled as an assertive pattern, while *B* is expected to respond to this with what is culturally regarded as submission, it is likely that this submission will encourage a further assertion, and that this assertion will demand still further submission. We have thus a potentially progressive state of affairs, and unless other factors are present to restrain the excesses of assertive and submissive behaviour, *A* must necessarily become more and more assertive, while *B* will become more and more submissive; and this progressive change will occur whether *A* and *B* are separate individuals or members of complementary groups." The other type is thus explained: "If, for example, we find boasting as the cultural pattern of behaviour in one group, and that the other group replies to this with boasting, a competitive situation may develop in which boasting leads to more boasting, and so on. This type of progressive change we may call *symmetrical schismogenesis*." Bateson gives illustrations of both types in our own culture in individual psychopathic behaviour, in the interrelation of social classes, in international relationships, and suggests that even within the individual there may be splitting of the personality thus induced or aggravated.

Ethos, as modified by, and operating through, schismogenic mechanisms goes a long way towards the final explanation of *naven*. A central consideration is the peculiar relation between the sexes among the Iatmul. Much of the boasting and display of the men seems to be directed towards attaining of admiration from the women, whom the former affect to despise. Similarly the secrecy practised in connexion with activities in the ceremonial house seems to be motivated by a desire to exclude women from knowledge as to what goes on there. Men affect an immunity to the painful emotions and so may make fun of women's dirging by caricaturing it. In general there is a complementary schismogenesis between the sexes. The ethos of the men becomes then one of exaggerated superiority. This is not adapted to the expression of vicarious pride, which belongs naturally to the female role. So in *naven* when the *wau* has to exalt the achievement of his *laua* he puts on women's clothes. Having done so he can show his despite for the other sex by wearing mean and filthy widow's weeds. Bateson's point is that the transvesticism emphasizes the inappropriateness of a male creature ever admitting that another male creature is a superior being. Similarly women when they are casually in the public eye are retiring. If they make a 'public appearance', however, they are behaving like men and so they put on the bravest masculine attire and carry themselves with pride. On the other hand when, during part of the *naven* ceremonial, they demonstrate the superiority of the *laua*, they lie down naked and have the *laua* step over them.

After some further explanations of *naven* as an expression of kinship motivation which are too complicated to be rehearsed here, Bateson goes on to *Eidos*. This is the cognitive aspect of function just as *Ethos* is its affective aspect. He defines it as "a standardization of the cognitive aspects of the personality of the individuals...and its expression in cultural behaviour". Among the Iatmul *eidos* is exhibited in two forms. There is stimulated an intellectualism unusual in a savage people which shows itself particularly in development of memory. Secondly, there are certain basic assumptions of what we should call a metaphysical character that mould the Iatmul's attitude towards nature, human, inanimate and spiritual, and thus affect not merely his beliefs but also his actual experience.

This résumé of Bateson's facts and arguments is unfair to the author as an anthropologist because of the reviewer's inability to select the data that are of anthropological novelty or importance. On the psychological side it does little justice to the amazing fertility of the ideas put forward, sometimes in extended argument, sometimes as hints that are fascinating in their potentiality. On the other hand the mere excerpts of Bateson's work that are given here may give an impression of unity which is certainly not derived from cursory reading. But this is to be expected for the author does not set out to write a coherent description of the Iatmul nor to adumbrate some central theory. The book is just what it says it is—an account of experiments in methodology. This account, being truthful, includes the unsuccessful as well as the useful methods of approach, the methods that are discarded or amended as well as those which are finally adopted. The latter are more or less brought together in a final chapter.

The experiment has undoubtedly been of great value to the author but whether it will have similar value to other anthropologists is a question which a psychologist cannot answer, because ability to answer it would depend on an intimate knowledge of anthropological evolution and its present status.

Certainly from the psychologist's point of view the almost autobiographical technique of presentation is unsatisfactory. An example will illustrate this. Bateson states that his realization of the existence of 'eidos' as a factor and as an important one came to him only when the work was largely completed. If what is to us peculiar in Iatmul culture is largely a product of a different 'philosophy', then an understanding of that 'philosophy' would prepare the reader for the assimilation of a large number of queer facts that are indigestible without such preparation. In a work of exposition *eidos* ought to come first, not last. The reader is made to repeat in small part the labour which the author went through in finding a pathway through the maze of apparently irrational and disconnected bits of savage behaviour.

Another, although minor, source of difficulty is Bateson's insistence on formulating all his principles in terms of the reactions of individuals. He inveighs against the 'mysticism' which is latent in the consideration of a society as if it had some existence apart from the reactions of the individuals composing it. One quotation is worth making: "I am inclined to regard the study of the reactions of individuals to the reactions of other individuals as a useful definition of the whole discipline which is vaguely referred to as Social Psychology. This definition might steer the subject away from mysticism." The atomistic phrasing this point of view forces on Bateson's set formulations produces clumsiness. Fortunately he is apt to forget this individualizing of social influences in the course of his argument and is thus able to discuss such a factor as *ethos* as if it were an isolatable entity among social influences (e.g. on p. 220).

As a further example of disturbance in the development of his thesis which comes from an autobiographical intrusion the discussion of Kretschmer's types may be mentioned. Having evolved his notion of the differing *ethos* in male and female, and after he had left the field, Bateson discovered Kretschmer. He tries to fit his subjects to Kretschmer's Procrustean twin beds and has to admit the classification is inadequate. The masculine *ethos* ought to correspond to the schizothyme temperament and the leptosome physique. Similarly the feminine *ethos* ought to go with pyknic body and cyclothyme personality (although he admits there is not the slightest evidence for circular emotions or behaviour in the women). The men being schizothyme, there is an effort made to correlate lapses from Iatmul observances (under white influence) with cyclothyme tendency. But in the one example he gives the culprit is schizothyme in make-up and pyknic in build. This is then at the same time an exception to Kretschmer's thesis and to Bateson's application of it. One feels after reading this chapter that the author has a vague hope that "something of this kind" might be useful but that it is an idea more suitable for fireside discussion than for inclusion in a serious anthropological treatise.

Perhaps a more serious defect is Bateson's exclusion of historical method. He says that he is engaged in making merely a synchronic study of Iatmul culture, that is, he considers in his explanation only those phenomena which are now operating and excludes the diachronic method which assumes that what is past may still be influencing present custom. This is equivalent to the omission of tradition as a formative influence. Bateson may, of course, be right. It is conceivable that the past has no hold on us as such, that it is only its phenomenal survivals—which are really present-day phenomena—that influence a culture. But such a view should not be adopted arbitrarily in a survey which aims at synthetic completeness. Yet this is what Bateson

does. The historical line of approach is mentioned only to be excluded. Apparently he feels that it is 'unscientific' or 'mystical' to attempt to deal with what is not available for direct scrutiny. As a matter of fact the absence from his speculations of any reference to a possible past history, of an evolution belonging to those features of Iatmul culture which are Bateson's problem, seems to the reviewer to be the gravest defect in this work. His analysis is ingenious to the point of brilliance but it is in the end unsatisfying because it is incomplete. He demonstrates the existence of *eidos*, *ethos* and *schismogenesis* and proves that they would tend to produce details of the culture which are demonstrably there. But behind these details one feels the existence of a general pattern, a basic theme whereon these details are merely variations and one suspects that he is, so to speak, writing an embryology of human neck and head organs that excludes consideration of gills.

This central pattern has to do with the peculiar status of women, something that informs the conflict between the patrilineal and matrilineal systems, that is inherent in much of *naven* and, indeed, crops up all the time as Bateson himself is repeatedly observing. There is a formal and obvious patrilineal system that is, so to speak, the 'official' one. But there is also a matrilineal one which many details of cultural behaviour seem to aim at upholding. The men, great swaggering warriors, claim to despise the women, yet the latter seem to have an anomalous power. In the *iai* marriage (in which a man marries his father's mother's brother's son's daughter) the woman makes the necessary advances and a man so chosen has no right to refuse. Women hold the purse strings and rule in many domestic matters. Quarrels are frequent and their issue may be the exclusion of the husband from bed, from board and even from the house. In other settings this man is a cruel bully; why does he not beat his wife into submission? Finally in some ceremonials when the women parade before a mixed audience they exhibit a proud bearing and a dignity never recorded in any of the men's ceremonials.

To a psychopathologist there is a tempting explanation for all this. It is simply that earlier in the history of the Iatmul there was a matriarchy that gave prominence to women not only genealogically but also in some kind of final authority in the community. This system broke down partially, patrilineal descent was introduced, but the men have never achieved what was taken from the women in the way of authority. There is too much swaggering about the men, too much insistence on the degradation of women, not to suggest that they are talking and acting 'behind teacher's back'. This hypothesis may be anthropologically preposterous although psychologically plausible and perhaps it is one of those ideas referred to above which are best published only to a fireside group. But, taking that risk, I should like to draw attention to its relevance to another problem. This is the question as to whether it is justifiable to speak of 'normality' or 'abnormality' in a culture, to use such terms as 'civilized' or 'decadent'.

Bateson says: "The business of the scientist is to describe relationships between phenomena, and any *ethos* which he finds in a culture must be regarded not as 'natural' but as normal to the culture. Unfortunately what is normal to one culture may well be abnormal to another, and the anthropologist has at his disposal only the adjectives and phrases of his own culture. Thus it has happened that English people with whom I have discussed Iatmul *ethos* have sometimes remarked that women appear to be 'well adjusted' while the men appear to be 'strained' and 'psychopathic'. My friends forget that the values

assigned by European psychiatrists to various mental conditions are either *cultural* values based upon European ethos or estimates of the fitness of the individual for life in a European community.' With this I agree so far as it concerns the normality or abnormality of the individual. But I do not see why a defeatist attitude about the relative status of cultures should be accepted. Why should we not adopt the biological criterion of success in adaptation? It is the task of the human, as it is of every other, species to adapt itself to the environment. The culture which can protect itself from the assaults of animals, disease and weather, and which can exploit natural resources, ought to be ranked 'higher' than is a culture which fails, relatively, in such endeavours. Secondly, dominance over nature brings with it a superiority to material needs that is reflected in the development of intellectual, moral, aesthetic and spiritual values. If one uses these criteria, how may the culture of the Iatmul be ranked?

Bateson tells us practically nothing about the economics of this people, so we have no basis for judgment as to their ability in subjugating nature. But the other criteria are available. In his discussion of *eidos* he gives ample evidence to show that the Iatmul have intellectual tastes. They do not exercise their minds over problems that appeal to us, but that is irrelevant. The important point is that they do place a premium on some kind of intellectual exercise as if it had a value in itself. In fact this development is so disproportionate as to suggest compensation. So larval is their religion that it can hardly be said to exist at all. Their spirits are vulgar, very human ghosts and their mythology seems to include little aspiration towards higher things. Similarly their art seems to be of a rather lowly order but perhaps it would compare well with that of peoples who are similarly isolated. But when we come to the moral sphere their ranking is abysmal. Bateson stresses over and over again the individualism which runs through the lives of the men; there are no laws, and conventions are relatively weak in the coercion of individual behaviour. Even their taboos are frequently broken. Loyalty of any kind seems to be almost unknown.

Now it seems to me that the weakness of moral values among the Iatmul may be correlated with what Bateson calls peripheral organization. There are no chiefs; their shamans traffic in benefits and hurts to individuals, they are not prophets who guide the people. There is nothing centralizing in the culture *except*, so Bateson argues, the matrilineal system which lurks in the background but inspires *naven*. Whatever may be the ultimate basis of morals there can be little doubt but that they grow with the altruism inspired by group co-operation. Is it not possible that at one time the Iatmul had a matriarchy with exogamy (of which Bateson reports traces) and the ethological standards that go therewith? This was displaced by a patrilineal system that has so far not evolved to the point of erecting a central authority to take the place of the deposed women. The men behave like spoiled children. If it is normal for adult men to be undisciplined, the ethos which decrees this to be normal reflects degeneracy—at least in the moral sphere.

Finally we come to the criticism of schismogenesis. Of this there are two kinds and it is questionable whether they ought to be grouped together rather than treated as quite different principles. In biology it is not customary to ally overgrowth and differentiation, and yet these are the biological analogues of symmetrical and complementary schismogenesis. (Further it does not seem that Bateson has made out much of a case for novelty in his invention of symmetrical schismogenesis. So far as the reviewer can see there is nothing here

that is not recognized by the layman as *rivalry*. Rivalry produces just that kind of accentuation of the activity subject to competition which Bateson describes with his new term. It would be surprising if anthropologists had not discovered this principle long ere this. This, however, does not mean that it is unimportant.) The settings of the two schismogeneses are, again, so different as to make their alliance of doubtful validity. In the complementary type there is a bond uniting together the two mutually interacting parties, a bond which the schismogenesis tends to rupture. In the symmetrical type the orientation of the two parties is outward; each is trying to impress outsiders, and the schismogenic development—if it has any effect on these bonds—tends to strengthen the ties with the outsider.

But this is all that can be said in adverse criticism of schismogenesis. Taking the complementary as the one, and real, schismogenesis, I think it possible that Bateson has with this concept forged a new tool for use in the biological sciences. He has indicated how useful it may be in the understanding of cultural problems; he has pointed out how it may illuminate problems in the fields of politics, sociology and social psychology; that it may be utilized in individual psychology he suggests and I believe his suggestion may be fruitful. But its utility may go far beyond this. His general formula is so reminiscent of events in the endocrine field that one wonders if it be not a principle that has operated in the differentiation of the organs of internal secretion. If there, why not in other fields of embryology and organogeny? Since the problems of ecology and sociology are similar when put into sufficiently general terms, it may not be irrational to hope that schismogenesis may have its application in this further biological field as well. Much will depend on the trend of development in biological theory in the near future. The time is ripe for attention to be turned again to general speculation as to living processes after a couple of generations spent chiefly in a frenzied collection of facts. So, whether Bateson's 'Naven' lives as an anthropological work or, having served a useful purpose during this generation, it dies, his 'schismogenesis' may survive as an important biological principle.

PUBLICATIONS RECENTLY RECEIVED

Introduction to Physiological Optics. By J. P. C. SOUTHALL. Oxford University Press: Humphrey Milford. 1937. Pp. x+426. 21s. net.

To those interested in the organs of special sense, Prof. Southall will be known as the editor of the English edition of v. Helmholtz's *Physiological Optics*, and as a physicist whose major interest lies in physical optics and in vision. The present book has been written primarily for the "large class of intelligent laymen who in different degrees and from various motives are desirous of obtaining such knowledge as is within their reach concerning the dioptries of the eye and the sense of sight" (Preface, p. v). It is difficult to feel that this aim has been achieved. Thus, whilst Chapter I, on the anatomy of the eye, is admirably lucid and simply expressed, certain parts of the following chapters on the optics of the eye, on errors of refraction and on spectacles, would surely be well beyond the reach of all but the more exceptional layman, or perhaps the specialist himself. These chapters will, however, be of value to the specialized student, for here will be found a discussion of the physical principles involved both in the eye itself and in the manufacture of spectacles, such as can be found elsewhere only in advanced books on such matters.

Certain criticisms must be made of the more physiological matter which follows. Chapter VII, on rod-mediated and cone-mediated vision, is distinguished by a remarkably uncritical discussion of Hecht's theory of intensity discrimination. Although in Chapter II Prof. Southall's account of visual acuity takes full note of the influence of diffraction of the retinal image, and indeed touches upon the probable effect of the monochromatic aberrations also, there is no mention of the far more important chromatic aberrations of focus and magnification. The reader will infer, as indeed is the case, that Hartridge's eloquent theory of visual acuity consequently receives no discussion whatever.

The penultimate chapter, on Theories of Colour Vision, is also unsatisfactory for many reasons. The significance of the phenomena of dichromatism for the tri-chromatic theory is discussed with more enthusiasm than accuracy, and the chapter, all too short in every other respect, concludes with a most complete and eulogistic account of Ostwald's peculiar colour system.

It is difficult to escape the conclusion that the author has come overmuch under the influence of the more plausible theorists in this field. His book, whilst of value in its treatment of certain aspects of physical optics, cannot be taken as an adequate or accurate account of the organ of vision.

A.F.R.-S.

Pavlov and his School. By Y. P. FROLOV. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1937. Pp. ix+291. 12s. 6d. net.

Prof. Frolov has attempted to write a popular exposition of the experiments and theories of Pavlov, and has added some biographical details, and paid glowing tribute to Pavlov as a man and a scientist. The book is certainly well meant, and many people will undoubtedly read it with interest and some profit. At the same time it is more than a little diffuse, and it could have been made both more interesting and more convincing had the author shown a little more scepticism about some of the hypotheses described.

Time and its Importance in Modern Thought. By M. F. CLEUGH. With a Preface by L. S. STEBBING. London: Methuen and Co., Ltd. 1937. Pp. xi+308. 12s. 6d. net.

The subject of this book is Time, and so nobody must complain that only one chapter of it is devoted to a study of the experience of temporal order, or that the

conclusions of the chapter are "we cannot ultimately remain satisfied with a purely psychological account of time" (not that anybody has ever really tried to give one), and "psychology stops just where things begin to become interesting". The chapter is a very good one so far as it goes. The author shows a scholarly knowledge and appreciation of the more classical psychological studies and some acquaintance with experimental work. That it achieves nothing new is a harmless criticism, for nothing of that kind is here attempted. The rest of the book is physical and philosophical, especially the latter.

Caste and Class in a Southern Town. By JOHN DOLLARD. Yale University Press. 1937. Pp. viii+502. \$3.50.

This book grew out of the author's original intention "to study the personality of Negroes in the South, to get a few life histories, and to learn something of the way in which the Negro person grows up". Finding that life histories cannot be collected "in a social void" (a "very unwelcome perception", as his time was limited to five months) Dr Dollard is compelled to study the "total situation" to which these life histories at every point refer—i.e. "Southerntown itself, the surrounding country, the south-eastern culture-area, and in a strict sense the whole region which is bound to the American cotton economy". He succeeds remarkably well, dealing with the relevant historical, economic, political, educational and religious issues in such a way as always to keep in the forefront the peculiarly psychological nature of his inquiry and backing up his interpretation by continual reference to the material he obtained from the taking of life histories. Thus, instead of a description of the linear development of any one Negro life, we are given a psychological explanation of the origin, maintenance and functioning of the caste and class relationships which characterize the community in which these Negroes live; and some indication of the way in which the structure of the community "patterns the affects" of white men and Negro alike. In so far as 'Southerntown', where Dr Dollard lived and worked, is "an average small town in a rural country devoted to a staple crop, and characterized by a black belt history and psychology", his conclusions will no doubt prove of great practical value to all students of the race problem in America; while his endeavour to supply and use psychological principles "keyed to our social science knowledge" will be of interest to psychologists and anthropologists and to the 'general reader'.

The basic assumption which underlies this attempt to pass beyond mere 'descriptive sociology' and to give a *dynamic* view of life in Southerntown, is that there are certain advantages and disadvantages which accrue to members of each caste and class, and that these advantages and disadvantages can *all* be resolved into *some* kind of 'personal, ultimately organic, gratification'. I am not quite sure what 'organic gratification' is; and I am fairly certain that (e.g.) such white middle-class 'gains' as the "satisfactions which go with mastery, superiority, control, maturity and duty well fulfilled" are not simply 'organic'. None the less, this concept of 'gains' does seem to be a fruitful one in so far as it yields broad general principles that have a certain 'predictive value'. Its fruitfulness, of course, is due largely to Dr Dollard's 'psychological insight', and to his admirable detachment. He is in fact almost *too* detached. For example, his eloquent description of the lower-class negro's opportunity and ability to enjoy "free direct impulse gratification", as compared with the middle-class American's "burden of ungratified longing which makes contented, day-to-day living impossible", makes one wonder whether, after all, it would not be better to be a lower-class negro satisfied than a middle-class American dissatisfied. The question, of course, is psychologically irrelevant, and Dr Dollard very properly contents himself with indicating that, as a matter of *fact*, "the dominant aim of our (American) society seems to be to 'middle-classify' all of its members", negroes included. By 'aim', Dr Dollard *may* mean simply a 'mass drive' over which we have no control, in which case nothing could be done about it. But if, as his words seem to imply, this aim is a real one, and at least partly 'in our power', then surely it is important to ask if we are aiming at the right mark. So far it is not at all clear *whose* business it is to answer this question. But I for one simply hate the idea of being 'middle-classified'!

Dr Dollard's "picture of the emotional underpinning which vitalizes social relationships in Southerntown" is always vivid and interesting, but it is frequently overcrowded and disorderly, and would gain considerably by being severely trimmed. Their love of 'facts' is such that American investigators often forget that one or two observations are sometimes sufficient to suggest and establish an hypothesis, and that little is gained by the multiplication of instances where neither the relevance of these instances to the principle in question, nor the principle itself, is clearly understood. In conclusion, would it be out of place to put in a plea for simple, direct English instead of so much cumbersome 'scientific' jargon? The worst of it is that one so quickly becomes 'sensitized' (or should it be 'desensitized'?) to the stuff.

D.M.C.

A Tribe in Transition: A Study in Culture Pattern. By D. N. MAJUNDAR.

London: Longmans, Green and Co., Ltd. 1937. Pp. xi+216.

10s. 6d. net.

This work can be heartily commended to the attention of any social psychologist who is interested in the effects of the contact of differently organized social groups, and who is prepared to make his study of these effects a matter of serious concern. It is written by a student of anthropology who has thoroughly assimilated the view that as an anthropologist it is his business to make a factual study of the internal and external relationships of the groups of people in whom he is interested and to try to present a picture of a living people and of the principles which, whether they are formulated or not, govern their changing behaviour. The people who are studied here are the Hos, who are concentrated in the Kolhan, an important part of the lower plateau of Chota Nagpur, and who are a fairly numerous and fairly well-localized population of this portion of India. Little has been published about them before, and that little Dr Majundar knows well. His own account is the result of several years of first-hand study. It is probable that a rather rapid first reading may give the impression that nothing very new emerges from this study, beyond additional concrete illustration of established principles of contact. This is because here, as in most places, the striking changes lie in readily observable directions, in dress, in building, and the like. But a more careful study will suggest, perhaps underlying these, and certainly in other directions as well, in social organization, relations of the sexes, and in the realm of beliefs, constant, and on the whole specifically directed, changes are continuing. Most of these are summed up in a final, and very clear, chapter entitled Cultural Contact and Adaptation. In this chapter, also, some extremely suggestive remarks are made concerning the mechanisms of diffusion of the effects of contact which are internal to the social organization of this particular people.

Most readers will perhaps regret the absence of a map, and most psychological readers will wish that the general observational data could be supplemented in certain directions by a more experimental or controlled technique of approach. But all the same the book is definitely a good one for a psychologist and will provide him with many topics concerning which he could collect for himself material from whatever social communities offer themselves to his study.

Personality and the Cultural Pattern. By JAMES S. PLANT. New York:

The Commonwealth Fund. London: Humphrey Milford. 1937.

Pp. x+432. 10s. 6d. net.

Current psychiatry tends predominantly to seek its explanations in terms of failures of the individual personality and of motives and drives inherent within that personality. The author of this book is a physician who has charge of a large children's clinic in America, and his many years of experience lead him to believe that equally or more important are influences springing from the environment. By this he does not mean merely of the material environment, but the influence of some dynamic pattern

of culture of which the single personality may be a partial, and in many cases, a very imperfect expression. In this book he sets himself out to study the ways in which cultural pattern may affect adjusted and maladjusted behaviour. All the argument is interesting, but unfortunately the great bulk of it is in very general terms, so that it is more often than not difficult to conclude how great a stress can legitimately be laid upon the factors whose operation is emphasized, or precisely how they operate in typical cases. Moreover, the whole notion of dynamic culture pattern does not really receive anywhere a sufficiently clear definition or explication. However the volume draws strong attention to the importance of social determinations which it is the fashion in many quarters either to ignore or to treat as of derived importance only, and in that respect it serves a most admirable purpose.

The Neurotic Personality of Our Time. By KAREN HORNEY. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1937. Pp. 299. 12s. 6d. net.

Dr Horney wishes to show that the essential characteristics of all neurosis are produced by difficulties existing in our time and civilization; that is to say, they are due pre-eminently to cultural, not to biological, factors. The dynamic centre of neurosis is anxiety, the main source of which lies in hostile impulses against others, resulting in fear of their retaliation and attempts to protect oneself against this by obtaining affection and approval, by submissiveness, by power, or by withdrawal. In our civilization such hostility and rivalry between persons is encouraged. Moreover, there is a premium on individuality and self-reliance, the over-development of which is liable to produce unendurable emotional isolation; hence the tendency of the neurotic to rid himself of loneliness by losing the self and dissolving the individuality in masochistic impulses and behaviour. The person likely to become neurotic is thus the one who has experienced these culturally determined difficulties in accentuated form, particularly in childhood.

This theory, which presents an interesting and suggestive alternative to the Freudian theory of the biological causation of neurosis, is clearly and ably developed by Dr Horney, in so far as it can be demonstrated from cases which are all, of course, subject to the same type of cultural factor. No final proof can be given until far more is known of psychological maladjustment in other cultures.

The Subnormal Mind. By CYRIL BURT. Oxford University Press. London: Humphrey Milford. 1937. Pp. ix+367. 10s. 6d. net.

This book was first published two years ago. That a second edition should have been called for in so short a time is a welcome sign of increasing interest in serious psychological problems which are discussed without any attempt to evade their more difficult issues. Prof. Burt has revised the whole of the original writing and made some corrections and additions. Among the latter a word list for free association tests suitable for use in this country will be particularly useful. It is now to be hoped that this edition will be exhausted as speedily as its predecessor, and that many others will follow.

Psychology of Personality. By ROSS STAGNER. London: McGraw-Hill Publishing Co., Ltd. 1937. Pp. xi+465. 21s. net.

In the Introduction to this book problems are stated and methods described. Generally speaking the view is adopted that personality is a collection or organization of characteristics which distinguishes one individual from another. While the possibility of a truly analytic study of these characteristics is considered, Prof. Stagner inclines to prefer methods which consider the interrelations of the separate traits concerned. The second section deals with the descriptive psychology of personality, which is concerned

with how personality appears to an onlooker who is considering its manifestations in various forms of behaviour and in the relations between these. The third section discusses the dynamics of personality, and considers in the main appetite and aversions and various theories of these. The part contributed to these by cultural environment is stressed. Finally comes a study of determinants of personality: the family, play and leisure time occupation, schools, economic conditions, culture patterns (Dr A. H. Maslow is responsible for a chapter on these) and social values. A bibliography follows.

While the book can hardly be said to break new ground, it does give an excellent and fairly concise description of much of the more important concrete work relevant to its topic of study. Each chapter is followed by a brief and lucid summary of its main contents, and by rather well-selected suggestions for further reading. A student in search of problems will find plenty to set him off in this book, while one in search of information will find, probably, less than he might hope, but about as much as he has any right to expect.

Personality: A Psychological Interpretation. By G. W. ALLPORT. New York: Holt. 1937. Pp. xiv + 588. \$3.50.

Although many books have been published with the word 'personality' in their titles, this would appear to be the first systematic treatment of the whole field in the English language. Most previous writers have considered the study of personality either as a branch of philosophy, or of medical psychology, or of mental measurement and applied psychology. But Prof. Allport follows Stern in regarding the individual personality as the basic datum from which the whole of psychology may be derived (and, be it added, his style is much more readable than Stern's). He points out that the traditional introspective schools of psychology concerned themselves with "the generalized human mind", and that modern investigators who claim to study "individual differences" are in fact dealing only with differences in test performances, in isolated mental functions, or statistical factors, not with individuals. Both approaches therefore tend to strike the layman and the young student as abstract and unpractical. Instead they should be subservient to the study of the individual person, considered as a unique entity.

This thesis possesses great attractions, and it is argued very thoroughly and lucidly. But it entails what will seem to many an acute drawback, namely, the denial that psychology can ever be a natural science in the orthodox sense. For science is always concerned with *classes* of objects, and *general* laws; and is at present incapable of dealing with the individual case. Nevertheless, the author believes that a wider conception of science is possible, which will aim to establish laws of individuality, without losing its objectively verifiable and experimental characteristics. Lack of space precludes any attempt to describe here his solution of this conflict.

The book includes an erudite account of the historical development of the terms personality, temperament and character, and of the various characterological doctrines (physiognomy, type psychology, etc.). It then takes up the genetic development of personality, showing how the study of maturation and learning and abnormal psychology may be fitted into the scheme. (The latter section might, in the view of the present writer, be considerably expanded. The contributions of Freud, and particularly of Jung, hardly receive the recognition they deserve.) A very full discussion follows on the nature of traits, and of other alleged 'elements' of personality such as specific habits, and factors. The various methods of studying personality—clinical, experimental, cultural, etc.—are then described and analysed; finally there are chapters on the outer expression of personality, and on the nature of our understanding of personality or intuition.

The field which Prof. Allport covers is one of outstanding interest both to contemporary American and to Continental psychologists; and his integration of their diverse efforts within this field is a considerable achievement. British psychologists have hitherto paid comparatively little attention to it; thus the book may be recommended to them even more strongly than to those for whom it was primarily written.

P.E.V.

Social Psychology of Education. By A. O. BOWDEN and I. R. MELBO. London: McGraw-Hill Publishing Co., Ltd. 1937. Pp. xv + 296. 15s. net.

The purpose of this book is intended to be the application of what is known about social psychology to the theory and practice of education. In fact, it illustrates well, in a popular manner, how an attitude of scientific detachment helps in showing up emotional prejudice about educational methods; and describes how various educational shibboleths should be carefully examined and accepted only with the greatest reserve. It is possible, however, that they may merely be replaced by a corresponding set of psychological shibboleths, unless a rather less superficial psychological treatment is adopted.

The book seems quite unnecessarily expensive for the amount of matter contained in it.

The Education of the Emotions. By MARGARET PHILLIPS. London: George Allen and Unwin, Ltd. 1937. Pp. 318. 8s. 6d. net.

The title of this book is somewhat of a misnomer. The author has in fact set out to study the development of 'sentiments' in the child and young adult; and her method has been to obtain from 275 adults of both sexes, all ages and various types of occupation, written accounts of the development of any interest or 'sentiment', "showing the stages of its development, the sources from which it derived, and the nature of the satisfaction obtained from it". 'Sentiments' were described for persons, things, social groups, intellectual interests, sports and crafts, aesthetic objects and pursuits, and various abstract ideals. And from these reports the author has produced an extremely interesting study of the gradual change and increasing complication in the linkages between cognitive apprehension and understanding, on the one hand, and emotional and impulsive tendencies, on the other. Whether in fact these linkages constitute structures so stable and well defined as to be called 'sentiments' is more doubtful; indeed the author herself refers to the "artificiality of the concept of separate sentiments". For it appears that not only will a given set of emotional tendencies attach themselves in turn to a bewildering succession of objects; but also one particular type of object will arouse successively very varied emotions. Moreover, there is no evidence to show that the emotions in themselves are in any sense 'educated' or altered; they are, rather, transferred and re-synthesized. But the book provides an admirable study of the change wrought by transference and synthesis as intelligence develops and the cognitive material becomes richer and less completely ego-centric. Moreover, the lines of change are not drawn purely at random; certain definite patterns emerge, which the author traces clearly in her material. Lastly, it appears that the process of change can only in the broadest sense be called one of education; since the educationist can best foster it by allowing it to proceed freely along its own lines; contenting himself with presenting suitable cognitive material rather than trying to inculcate knowledge or emotional attitudes.

M.D.V.

What Man has Made of Man. By MORTIMER J. ADLER. New York: Longmans, Green and Co. 1937. Pp. xix + 246. \$3.50.

Dr Adler gave four lectures at the Institute for Psychoanalysis in Chicago. Here he reprints these in the "form in which they were given". This form, which is like a set of rather full notes, makes them extremely difficult to read with much pleasure. There are 123 pages of lectures, and the rest of the book is filled up with Notes on the lectures, many of them very long ones. Dr Adler asserts that he thinks that psychology is the most important of all modern topics of study, but this appears to be because he thinks that here science and philosophy meet. He makes it clear that he has very little

use for most developments of psychology, except his own particular interpretations of psychoanalysis, and, in a somewhat reluctant and limited way, for psychometrics. He is immensely pugnacious, dogmatic and provocative, and anybody who wants to find windmills to tilt at will have no difficulty if he looks at this book. The subtitle is *A Study of the Consequences of Platonism and Positivism in Psychology*, and in fact philosophy and psychology play battledore and shuttlecock throughout these pages in a most bewildering fashion. Dr Adler is much influenced by Scholasticism, but maintains that his brand is far from that of the Middle Ages. An Introduction is contributed by Dr Franz Alexander, and not a few people may find this, though it is short, the most interesting part of the book. Dr Alexander makes no secret of the fact that he is exceedingly dubious of most of what Dr Adler has to say. The book may serve the useful purpose of provoking a lot of people to disagreement, but from a constructive point of view it is not very helpful.

Common Neuroses of Children and Adults. By O. SPURGEON ENGLISH and GERALD H. J. PEARSON. London: George Allen and Unwin, Ltd. 1937. Pp. 320. 7s. 6d. net.

This book, which is very moderately priced, is intended as an aid to the medical practitioner in recognizing the commoner neuroses and personality disorders of children and adults; in understanding their psychogenesis; and in providing such treatment as is possible. The aim is admirable, and so also is the scheme of the book: it begins with a short general treatment of the structure of personality and of its pathological trends; describes clearly and briefly, with illustrative case histories, the commoner neuroses of children and adults; and concludes with advice on the conduct of the psychiatric interview. But the whole argument of the book is based upon an uncritical acceptance of the validity of the Freudian theory and practice of psychoanalysis. No other explanations of psychopathic states or symptoms are admitted or discussed than those provided by the theory of infantile sexuality. We cannot help feeling that the medical practitioner would benefit from a broader treatment of the subject of psychopathology both as regards his general understanding of it, and also as regards his ability to deal with practical cases. Indeed, the authors themselves seem to suggest an almost uniformly gloomy prognosis for cases diagnosed and treated on the lines they suggest: "All hope abandon, ye who enter here."

Introduction to the Rorschach Method. By S. J. BECK. Research Monograph No. 1, The American Orthopsychiatric Association. 1937. Pp. xv+278.

This monograph will be extremely useful to all those who wish to make use of the Rorschach ink-blot test. It consists mainly of case studies worked out in detail from the test results for eight classes of people: healthy adults of superior intelligence, mental deficient, depressed states, hypomanic conditions, schizophrenics, neurotics, conduct disorder cases in adults, and problem children. In addition, there are full and clear accounts of technique and scoring; and an admirable table of the psychological processes indicated in each of the above types of case by their responses to the ink-blot. But anyone interested in the assessment of personality will profit by a study of the experimental records, and will be impressed by the delicacy and flexibility of the technique and its clearness in diagnosis. Even more convincing would be a comparison of the clinical records of some of these cases with the Rorschach test diagnoses.

Der Selbstmord. Von R. WEICH BRODT. Basel. 1937. S. 250. Schw. Fr. 19.

Prof. Weichbrodt has written a very comprehensive monograph with skill and conciseness. His first mode of approach is historical. He deals with suicide in primitive communities, among the ancient Jews, the Greeks, the Romans, the Germans, among

the adherents of various forms of Eastern religion, in Christian communities generally, and among people without any particular religious belief. He discusses also some views of philosophers and poets about suicide, and some general legal considerations.

His next section deals with *Selbstmordstatistik als Moralstatistik*. He clearly summarizes a mass of fairly recent statistical inquiry in many civilized groups showing something about the frequency of suicide in different social classes and sub-groups.

Biographical remarks are offered concerning a number of more or less famous suicides, and finally the views of ten famous people about suicide are translated and set forth in brief. The book contains a mass of interesting factual material and comments thereon.

Medico-Legal Aspects of the Ruxton Case. By JOHN GLAISTER and JAMES COUPER BRASH. Edinburgh: E. and S. Livingstone. 1937. Pp. xvi+284.

Most readers in this country will remember that in the autumn of 1935 a number of human remains were discovered in the bed of a stream about two miles north of Moffat in Scotland. These were the basis of a famous case which eventuated in the execution of Dr Ruxton for a double murder. Very nearly all the most important points in the evidence were founded upon a most careful and meticulous medical reconstruction and analysis. In this book the mode and the results of this analysis are presented carefully, without sensationalism but with extreme clarity. The volume will be of absorbing interest to any psychological student who has a sufficient physiological and medical background. It is addressed to no others and to no others can it be either of great interest or even very intelligible. The account given is illustrated by many photographs, all of them extremely well reproduced. As a study of the slow but overwhelming building up of medical evidence in what at first was a most puzzling and difficult case, the volume should become a classic.

The Criminals we Deserve. By HENRY T. F. RHODES. London: Methuen and Co., Ltd. 1937. Pp. 257. 7s. 6d. net.

Mr Rhodes belongs to the Institute of Criminology of the University of Lyons. He has produced a hair-raising book, most interesting to read. Society, he thinks, is producing criminals on all hands, then detecting them, and then making them worse by stupid methods of punishment. He illustrates his thesis from nearly all the major and many of the minor branches of crime, and perhaps gives the impression both that there are far more criminals than most people think and also that it is a wonder that there are not a very great many more still. At the same time the book does make many valuable suggestions towards reform.

The Conquest of Violence. By BART. DE LIGT. With an Introduction by ALDOUS HUXLEY. London: George Routledge and Sons, Ltd. 1937. Pp. xi+306. 7s. 6d. net.

Many people will agree with the author of this book as to the dangers in the use of violence, and the desirability of adopting some new technique in dealing with the violence of others. But there will also be many to disagree with his tendency to attribute hostility and aggression solely to the 'wicked bourgeois'. There seems to be little evidence that the workers of the world can unite in anything but counter-aggression against repressive forces, or counter-repression of the holders of unwelcome ideologies. Yet as long as they use hostility to meet hostility, and unite only in aggression against aggression, will not violence be the inevitable outcome? The author seems to hope that the 'new psychology' and the 'new education' may eradicate such fundamental

hostility. But he devotes a negligible amount of space to the discussion of the problem as to how this may be done, though upon its solution all other pacifist projects must ultimately depend. Perhaps he realizes that at the moment the solution is little more than a pious hope.

Art and Understanding. By MARGARET H. BULLEY. London: Batsford, Ltd. 1937. Pp. xix + 292. 15s. net.

The work of Mrs Bulley is already very well known to English psychologists, if only from her extraordinarily interesting little book called *Have you Good Taste?*, which was published a few years ago, and from her attractive articles in various psychological periodicals. This is a more ambitious and complete study of problems appertaining to artistic creation and understanding than she has attempted hitherto, and it is so good that it should give her both a wider public and an enhanced reputation. Although the book contains a full and generous recognition of the part which psychology may play in clearing up many difficult problems of artistic production and appreciation, there is no narrow adoption of any particular point of view. In fact Mrs Bulley is critical and rightly critical of many of the things that have been said and written in the name of the psychology of art. To her, art is technique but also much more. It is the expression of sensorial and perceptual pleasure, but it is also much more. It is affected by fashion, whim, prejudice; it has its functions, biological and other. But by studying these its whole secret can never be revealed. Art is a revelation of something real. There is not good and bad art, but only good art and counterfeit. "The stuff of art is made out of the stuff of life and this is known to the creature through (not by means of) deep feeling." Nevertheless art, though it may be known through feeling, is penetrative Idea. Mrs Bulley has much to say about the Idea, its sources, the medium in which it can be expressed and the technique of its expression.

The book is divided into four parts. Part I is called *General*, and discusses in nine sections a variety of very wide problems of artistic experience and creation. In this part the effort is to provide the reader with a general philosophical and psychological background. No psychologist will probably agree with everything, but equally no psychologist could read what is written without being stimulated and without being brought up against many very attractive themes for thought. Part II is entitled *Particular*. Here are discussed, with abundant illustration, problems of individual and race psychology, of medium, craft, technique, function, subject matter, associated ideas, form, design and illustration. Here the psychologist with an experimental bent will find plenty of suggestions which may lead him into fruitful research. Part III has to do with *The Study and Criticism of Art*, and here too the psychological reader will find suggestions which he could well follow up by directed experiment. A short section on *Modern Art* is followed by an exceedingly interesting *Commentary on the Illustrations*, and this by a very delightful *Appendix* in which are collected quotations and comments on matters dealt with in the text. The Plates, of which there are 275, illustrating very many features of picture and design, are both well chosen and well reproduced. Altogether Mrs Bulley has produced a book which should take a high rank among British contributions to the study of art.

A word should be added about the actual form and printing of this volume. It is, indeed, a marvel of cheap production, a delight to read and to look at, well worthy of the material which it contains.

The Voyage to Illyria. A New Study of Shakespeare. By KENNETH MUIR and SEAN O'LOUGHLIN. London: Methuen and Co., Ltd. 1937. Pp. 242. 7s. 6d. net.

This book is announced as "a psychological biography of Shakespeare". The authors also say that they draw much of their inspiration from modern psychology though they do not desire to subscribe to any of its particular dogmas. Their main aim,

at any rate, is to try to present Shakespeare, through his works, as a person, rather than to consider the work as if it were a sort of generalized product of an impersonal artist. Whether their picture is accepted or not they may be fairly said to have succeeded in their aim, and to have unfolded an intense drama of individual human experience. As to the actual course and culmination of this drama they do not differ greatly from many other writers, but the way in which they put everything into direct personal terms makes the story more than usually exciting. They write not only with assurance but remarkably well, and for those who do not care for the personal picture there are masses of magical quotations.

FOURTH ANNUAL INTERNATIONAL CONGRESS FOR THE UNITY OF SCIENCE

THE Fourth Annual International Congress for the Unity of Science will be held at Girton College, Cambridge, England, from 14 to 19 July 1938. The chief topic will be the unification of scientific language. A special session will be devoted to fundamental problems of objective psychology and to psychological terminology. The congress is organized by the International Institute for the Unity of Science, 267 Obrechtstraat, The Hague, Holland.

UNIVERSITY OF VIENNA, PSYCHOLOGICAL INSTITUTE

THE Seventh Annual Summer School in Psychology, consisting of seven courses for English-speaking students, will be held from 11 July to 6 August 1938. Information regarding courses, etc., may be obtained from: Psychological Institute, University of Vienna, 1. Liebigasse 5, Vienna, Austria; or Austrian State Travel Bureau, 159-169 Regent St., London, W. 1.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

1937

GENERAL MEETINGS

- March 13, 1937. Papers:
"Psychological Analysis of Ideal Construction." By H. J. KERRIDGE.
"Some Aspects of Play Behaviour in Children." By RABINDRANATH GHOSH.
Demonstrations:
"Apparatus for the Study of Dark Adaptation." By L. R. PHILLIPS.
"Tests of Artistic Appreciation." By H. R. M. DEWAR.
- April 16-20, 1937. Extended General Meeting at Manchester.
- April 16. 1st Session:
"Social Classes and Social Psychology." By H. E. O. JAMES.
"Psychological Data and Physiological Processes." By A. D. RITCHIE.
2nd Session:
"Conceptions of Mental Fatigue." By C. S. MYERS.
- April 17. 3rd Session:
Symposium on "*Gestalt*".
Papers by: A. W. WOLTERS.
C. SPEARMAN.
4th Session:
Visit to the Psychological Laboratory of the University.
Paper:
"Visual Imagery in Directed Thinking." By T. H. PEAR.
Demonstrations by:
F. W. BAKEWELL.
M. DAVIDSON.
H. H. FERGUSON.
P. W. KIPPAX.
R. H. MANSON.
W. E. MOORE.
- 5th Session:
"The Analytical Error in Psychology." By EDGAR RUBIN.
"Taste Problems—A Proposed Enquiry." By H. H. FERGUSON.
- April 18. 6th Session:
"The Psychology of Problem Solving." By K. DUNCKER.
7th Session:
"A Thesis concerning the Fundamental Structure of the Mento-Emotional Processes in Children." By MARGARET LOWENFELD.

April 18.

8th Session:

"Culture Types and Psychology." By S. F. NADEL.

"The Work of the Bureau of Human Heredity." By C. B. S. HODSON.

April 19.

9th Session:

Discussion on: "What is a qualified Psychologist and for what work should he be qualified?"

Papers by:

R. B. CATTELL.

WM. STEPHENSON.

H. W. OLDHAM.

S. J. F. PHILPOTT.

10th Session:

"The Management of Workpeople." By C. A. OAKLEY.

"Commercial Judgments." By HENRY BINNS.

11th Session:

"Speech Therapy of the Mental Defective." By MARGUERITE NIELKA.

"Psychological Difficulties in 'Institutionalising' Defectives." By I. M. LAIRD.

12th Session:

Open Session for Discussion.

July 3, 1937.

"Gestalt Theory and Aesthetics." By HELEN KNIGHT.

"Studies in Fluctuations in Emotional States." By MADELINE KERR.

"Matching Technique applied to the Mosaic Test." By MADELINE KERR and P. M. TRAILL.

"The Kretschmer Psycho-Biogram and Q-Analysis." By R. J. BARTLETT and B. W. F. ARMITAGE.

October 29, 1937.

General Meeting, followed by Lecture. (See under LECTURES.)

November 20, 1937.

General Meeting at Reading.

"Influence of Meaning on Children's Drawings." By H. W. OLDHAM.

"Some Aspects of the Problem of Dark Adaptation." By L. R. PHILLIPS.

"An Investigation of Moral and Social Attitudes." By S. BRAHMACHARI.

December 11, 1937.

Annual General Meeting.

Discussion on "The Psychologist and Mental Abnormality."

Opening Papers:

"The Function of the Psychologist in the Mental Health Services." By D. N. HARDCASTLE.

"The Psychologist and Mental Defect." By C. J. C. EARL.

LECTURES

- February 26, 1937. "Psychological Problems of Television." By T. H. PEAR.
 April 29, 1937. "Hypnosis, Suggestibility and Progressive Relaxation." By
 WILLIAM BROWN.
 October 29, 1937. "Development and Personality in Childhood." By CHARLOTTE
 BÜHLER.

SECTIONAL MEETINGS

MEDICAL SECTION

- January 27, 1937. Address from the Chair: "The Frontiers of Psychotherapy." By
 H. CRICHTON-MILLER.
 February 24, 1937. Joint Meeting with the Education Section.
 Symposium on "Sex Education", introduced by SUSAN ISAACS.
 Papers by: LYN HARRIS (for the Education Section).
 W. R. BION (for the Medical Section).
 March 24, 1937. "Suicide." By A. B. STOKES.
 April 28, 1937. "Biological Echoes in Clinical Psycho-Therapy." By JANE
 SUTTIE.
 May 26, 1937. "Relations between Dissociation and Repression." By WILLIAM
 McDUGALL.
 June 23, 1937. "The Approach to Asthma." By J. L. HALLIDAY.
 October 27, 1937. "The Dilemma of Psycho-Analysis." By JOHN MACMURRAY.
 November 24, 1937. "Graphology as a Personality Test." By M. J. MANNHEIM.
 December 15, 1937. "The Meaning of Ritual." By K. de B. CODRINGTON.

EDUCATION SECTION

- January 7, 1937. "Health, Conscience and the Freedom of the Will." By WILLIAM
 BROWN.
 February 24, 1937. Joint Meeting with the Medical Section.
 Symposium on "Sex Education", introduced by SUSAN ISAACS.
 Papers by: LYN HARRIS (for the Education Section).
 W. R. BION (for the Medical Section).
 April 26, 1937. "Experimental Work in the Junior School." By C. FLETCHER.
 May 31, 1937. "Psychological Gleanings from the Montessori Method." By
 C. A. CLAREMONT.
 October 25, 1937. "Intelligence Tests in Junior County Entrance Examinations."
 By S. F. J. PHILPOTT.
 December 4, 1937. "The Values and Limitations of the Senior School and the Non-
 Selective Central School."
 Papers by: R. H. DUCÉ.
 W. VICKERS.
 H. G. STEAD.

INDUSTRIAL SECTION

- January 18, 1937. "Some Psychological Considerations of Questions and Answers." By G. H. MILES.
- February 22, 1937. "Psychological Aspects of Accident Prevention." By H. M. VERNON.
- April 12, 1937. "An Experiment in the Furniture Trade." By FAYE ORDE.
- May 20, 1937. "Incentives." By C. A. MACE.
- June 10, 1937. "The Problems of Class Stratification." By R. H. THOULESS.
- November 3, 1937. "What is Work?" By D. W. HARDING.
- November 22, 1937. "Some Educational Experiments with Unemployed People." By G. H. ROBERTS.
- December 13, 1937. "The New Factory Act." By D. R. WILSON.

AESTHETICS SECTION

- February 2, 1937. "Chinese Writing—A Gap in Western Aesthetics." By JOHN ROWDEN.
- July 14, 1937. "Idealism and Realism in Spanish Art." By F. D. KLINGENDER. (Cancelled owing to small attendance.)
- November 2, 1937. Special Meeting, to consider and vote upon the motion that the Section be abolished.

BRANCH MEETINGS

Session 1936-7

SCOTTISH BRANCH

- November 7, 1936. (Edinburgh.)
 "Suicide." By D. K. HENDERSON.
 "Mental Deficiency and Psychological Development." By I. M. LAIRD.
- December 5, 1936. (Glasgow.)
 "A preliminary account of a repetition of Dr J. B. Rhine's work in Extra-Sensory Perception." By S. G. SOAL.
 "Some Experiments in Telepathy." By A. CLASSE.
- February 6, 1937. (Edinburgh.)
 "Some Experiments on the Growth of Perception." By T. MURRAY MARTIN.
 "The Present Position in Intelligence Testing." By P. E. VERNON.
- March 6, 1937. (Glasgow.)
 "Prolegomena to a Psychology of Art." By W. R. D. FAIRBAIRN.
 "The Concept of the Imagination in Contemporary Aesthetics." By T. E. DICKSON.

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- May 8, 1937. (St Andrews.)
 "A Practical Introduction to Experimental Psychology." By
 B. BABINGTON SMITH.
 "Play in Early Childhood." By A. H. BOWLEY.

MIDLAND BRANCH

- November 25, 1936. "The Application of *Gestalt* Theory to the Psychology of Action." By M. HAMMOND.
January 13, 1937. "Dickens and Child Psychology." By B. EDGELL.
February 24, 1937. "The Child of the Future." By C. L. C. BURNS.

NORTHERN BRANCH

- April 16-20, 1937. (Manchester.)
 Extended General Meeting of the Society. (Arranged with the
 co-operation of the Northern Branch.) (See under GENERAL
 MEETINGS.)
June 5, 1937. (Leeds.)
 "Some Factors in Arithmetical Ability." By J. ROOUM.
 "Success in Junior Scholarships and School Certificate Examinations in the Light of Psychological Factors." By M. ORMISTON.
October 9, 1937. (Rotherham.)
 "The Essay in the Junior Scholarship Examination." By R. K. ROBERTSON.
 "Mental Deficiency and Social Class." By H. M. COHEN.
 "Hope and the Mental Defective." By F. J. S. ESHER.